BOYLE

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About the

SALTNESS of the SEA:

An Account of a

STATICAL HYGROSCOPE

And its USES:

Together with an APPENDIX

about the

FORCE of the AIR'S MOISTURE:

A FRAGMENT about the

NATURAL and PRETERNATURAL

STATE of BODIES.

By the Honourable *ROBERT BOYLE.*

To all which is premis'd

A SCEPTICAL DIALOGUE

About the POSITIVE or PRIVATIVE

NATURE of COLD:

With some Experiments of Mr. *BOYL'S* referr'd to in that Discourse.

By a Member of the *ROYAL SOCIETY.*

London, Printed by *E. Flesher* for *R. Davis* Bookseller in *Oxford*, M DC LXXIV.

AN

ADVERTISEMEMT

OF THE

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THE Reader of the following Dialogue may easily conclude from the beginning of it, (where the occasion of the Conference is set down) that

if the Author had been ſo minded, it might have long ſince come abroad. But though, as his backwardneſs to publiſh it kept it long lying by, firſt in His hands, and then in Mine; yet the Affinity it has with ſome of the enſuing Tracts of *Mr. Boyle*, and ſome other of his Papers that he deſign'd (but was hinder'd) to have added to them, engag'd me to take the liber-

iberty of publiſhing it and hem all together: Which I ad ſooner done than now I lo, if, by ſome Accidents, hey had not been kept from ppearing for many weeks af- er they were quite printed off.

A

OF THE POSITIVE OR PRIVATIVE NATURE OF COLD.

A Sceptical Dialogue

Between

Carneades, Themistius, Eleutherius, Philoponus.

SECT. I.

Eleu. MAy one be allowed to ask *Carneades*, what Book it is he is reading with so much attention?

Carn. The Question, *Eleutherius*, is very allowable, and as easily answer'd, by saying, that what I was reading, is our Friend Mr. *Boyle's* newly published *History of Cold*.

Themistius. Your readiness, *Carneades*, to an swer, encourages me also to ask you a Question which shall not be, as probably you expect i should, How you like this new Piece? for I know you would be too kind to the Author, not to tell m that he has detected some Old Errours, and mad discovery of some New Truths; but my Questio shall be about what is my Wonder, as well as tha of divers others, who think it strange that a Write that has deliver'd so many Effects and other *Phæ nomena* of *Cold*, should omit to tell us so much whether he asserts it to be a *Positive* Quality, or bare *Privation* of Heat; as, since *Cardan* (in h Treatise *De Subtilitate*) some other Learned Me and especially *Cartesius*, hath maintain'd.

Carneades. You will not wonder, if a Person th you look upon, and I confess not injuriously, a Friend to Mr. *Boyle*, tell you, that this Autho by the many Histories he has presented us, and b his not seeming to dare to determine the Contr versie you have mention'd, shews, that he w more sollicitous to lessen his ignorance, than pretend to knowledge: And upon the observatio I have made of his humour in general, I presum one principal reason of his silence may be, that h has not yet compleated the trials he had design about *Cold*; and thinks, that in Abstruse Subject such as this is, 'tis not so convenient to deliver positive opinion of the Nature of it at the Begi ning, as to reserve it for the latter End, after t History of the *Phænomena*; when the nature the thing inquir'd into may, as it were, spontan ously Result from the Considerations suggested the precedent matters of Fact survey'd together.

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Eleutherius. If such a wariness were indeed the motive of your friends silence, I shall easily excuse it; and perhaps think too, that the like would not mis-become Naturalists on many other occasions. And yet I do not dislike *Themistius*'s question; for 'tis one thing to venture upon declaring the adæquate Nature of Cold, and another to determine, Whether it be a Positive, or a Privative Quality? The latter attempt importing a much less venture than the former.

Carneades. I will not pretend to know the very Reasons that induc'd the Author silently to pass by this Controversie; but having been once present, when he had occasion to discourse of it, I then conjectur'd, that among his Experiments of Cold, that are not yet publish'd, there may be some uncommon ones, that may have suggested to him scruples, which oblig'd him to forbear declaring himself, till he had clear'd them, which those that are unacquainted with such Tryals, may probably have never thought of.

Themistius. If what you call a Controversie, were indeed worthy of that name, I should not unwillingly allow of your Friends silence; but the Opinion broach'd by *Cardan*, and adopted by Mr. *Des Cartes* and others, seems to me so devoid, not only of reason, but of all appearance of it, that me thinks one that has deliver'd such considerable Effects of Cold, as Mr. *Boyle* has done, may well ascribe to their cause, at least, a *Positive* Nature; and, without at all being guilty of boldness, reject an Opinion, that is not only barely an Errour, but an Extravagance, and perhaps a plain Absurdity.

Carneades. Possibly the Gentleman we are speaking of, may be wary and sceptical enough to reckon among difficult things, not onely the declaring the adæquate Nature of Cold, and the manner of its Operations; but the demonstrating whether it be a *Positive* Quality or not. And though I will not take upon me to know his thoughts about that subject, which, perhaps, are grounded upon some of his peculiar Experiments and Notions; yet, for discourse sake, I am content to debate with *Themistius*, Whether or no the Opinion he so severely censures, be not only erroneous, as, for ought appears, Mr. *Boyle* himself may be found to have thought it; but also, as *Themistius* would have it absurd.

Themistius. I readily accept of your offer; for it cannot be an unpleasant entertainment to observe the arts whereby one that I know will not speak impertinently, will endeavour to make Reason elude the clearest Testimonies of Sense. And though I might press you with the concurrent authority of *Aristotle*, and all the Philosophers that have liv'd etween his time, and those of that extravagant ellow *Cardan*; yet I shall rather employ, to convince you, the authority and reasons of a grand Leader among your New Philosophers, who being a great broacher of Paradoxes, and having upon that score written Books expresly against *Aristotle* was not like to have sided with him, unless the Evidence of Truth had, as it were, necessitated him to do so.

Carneades. I presume, you mean the Learn'd and Subtle *Gassendus*, whom I am glad you have pitch'd upon for your Causes Champion, not only becaus

n defending the common opinion, he waves the ommon practice of troubling his Readers with a ultitude of Authorities, which to me, in such a ase as this, would signifie very little, and betakes imself to arguments; but because, being so moern and judicious a Writer, we may well suppose im to have summ'd up and improv'd what can be id in behalf of the cause he maintains. Upon hich account, I shall be excus'd from answering npertinent Objections against the Opinion I deend, and from the trouble of ranging about among ther Authors for more weighty Arguments than ose, which the disproving of his will shew to be nsatisfactory.

Themistius. I am glad you nam'd the Author I eant, *Carneades*; for I apprehended you had not et with what he sayes upon this subject: because could scarce imagine, that an intelligent person, fter having read his arguments, will doubt of a ruth he hath so clearly evinc'd by them. But since perceive you have seen what he has written, I hall, without farther preamble, propose his Reons to you, though not in the very same order herein he has couch'd them.

Eleutherius. But before you begin them, give me ave to ask *Carneades* a short question, whose anver will, I suppose, conduce, if not be necessary, o the clearing of the state of the Controversie bewixt you. For 'tis one thing to deny belief to the eceiv'd Opinion, that Cold is a Positive Quality, nd another thing to assert, that 'tis but a Privation f Heat; since, if *Carneades* does undertake the tter of these two, he must bring positive Arguents to prove Cold to be but a negative thing.

Whereas, if he content himself to play a doubting part, it may suffice him, being in effect but a Defendant, to shew that the proofs brought to conclude Cold to be a Positive Quality, are not Cogent.

Carneades. I acknowledge your Question, *Eleutherius*, to be pertinent, and not unseasonable. And I presume, you will not be surpriz'd, that a Person accus'd of Scepticism answers it by declaring, that he undertakes not to demonstrate, that *Cold* must be a Privative or Negative Quality, and thinks it sufficient for his turn, to shew that the Arguments brought to evince it to be a Positive one, are not concluding. And, since you have already diverted *Themistius* from beginning so soon as he intended 'twill not be amiss, that I continue that suspension a little longer, to prevent, what I know we both hate, Verbal Controversies; which yet may very easily spring from undetermin'd acceptions of Words as ambiguous as I have observ'd Heat (of which I now make Cold but a Privation) to be.

We may therefore consider, that the word *Heat* being made use of to signifie, as well the operations of that quality upon other Bodies (as when the Heat of the Fire makes Water boyl, or that of the Sun melts Wax, and hardens Clay) as its operations upon the Sense of man, (as when a moderate degree of Heat is said to cause pleasure, and an excessive one to produce pain;) this Term, I say as Mr. *Boyle* also has somewhere noted, may be employ'd sometimes in a more absolute and indefinite sense, and sometimes in a more confin'd and respective sense: In the latter of which, 'tis estima-

ted

ed by its Relation to the Organs of Feeling of hose men that judge of it. Upon which account, hen are wont to esteem no body Hot, but such an ne, the agitation of whose small parts is brisk e- ough to increase or surpass that of the particles of he Organ that touches it: For if that motion be nore Languid in the Object than in the Sentient, he Body is reputed Cold; as may appear by this, hat if the same Person put one of his hands when is hot, and the other when 'tis cold, into luke-warm Water, that Liquor will feel cold to the warm hand, nd warm to the cold.

Eleutherius. So that according to this Doctrine, nethinks, one may, for brevities sake, convenient- y enough apply to your two-fold Notion of Heat, hose expressions which some School-men employ bout certain Qualities, of any of which they say, hat it may be either materially or formally consi- er'd. And by Analogy to their Doctrine, since *Heat* is a Tactile Quality, and as such, imports rimarily a relation to the Organ of Touching, that elation, with what depends upon it, may pass for hat which is the *Formale* in the Quality called eat; and its Effects and Operations upon other odies may supply us with a Notion of Heat, ma- erially taken.

Carneades. I do not always quarrel, *Eleuthe- ius*, with Terms borrow'd from the Schools, if they e as much more short and expressive than others, s they are more unusual, or even barbarous. But here is another Distinction of *Heat*, partly ground- d upon that already propos'd, which, because it nay be of use in our future Discourse, will not be nfit to be here intimated. For we may consider,

that though, for the most part, a hot Body is taken in the vulgar sense for that wherein the degree of Heat is sensible to our Organs of Feeling; yet it a looser sense, and which, for Distinctions sake we may call Philosophical, because concluded by Reason, though not perceiv'd by sense, a Body may be conceiv'd not to be destitute of Heat, even when the degree of that Quality is not great enough to be felt by the Touch; provided it can produce in some degree those other Operations, which when more intense, are acknowledged to proceed from manifest Heat. For elucidation of which, we may alledge, That in very frosty, and yet clear, Weather, the Sun may be judg'd to warm the Air, when it melts Snow, and thaws Ice; though, perhaps many men, especially of tender Constitutions, feel in their Fingers and Toes much stiffness and more pain, upon the account of Cold. To this I may add the common Observation, if you grant the truth of it, that Snow melts much sooner upon Land newly turn'd up by the Plow, than, *cæteris paribus*, in the neighbouring ground; which argues a warmth in that newly expos'd earth: though according to the Touch it would questionless appear Cold. But we may be furnish'd with a clearer and more pregnant Instance, by but recalling to mind what was just now mention'd of the warmth of tepid water, which was not to be felt by a hot hand, but produc'd there a contrary sensation of Cold. Which Instance I therefore scruple not to repeat, because it affords an Experiment in favour of that premis'd Distinction, which, I think, may also have this ground in Reason, that a considerable Heat is often requisite to be sensible to our

hands,

hands, *&c.* which are continually irrigated with the Circulating Blood that comes very warm out of the Heart, and enliven'd by Animal Spirits, plentifully supply'd from the Brain.

If *Eleutherius* think fit to accommodate this Distinction in the Vulgar and in the Philosophical sense to his *Heat*, formally and materially taken, I leave him to his liberty. And I shall also leave it to you both, Gentlemen, to accommodate to Cold, *mutatis mutandis*, as they speak, what has been said about the distinctions of Heat; because, I fear, *Themistius* thinks himself to have been too long detain'd already from proposing his Arguments, which he may now begin to do assoon as he pleases.

SECT. II.

Them. I Will then, with your permission, begin with that Argument of *Gassendus*, which I am able to give you in his own words; because upon the occasion of Mr. *Boyle*'s book, I made a Transcript of what he sayes to evince the *Positive* Nature of Cold; and having the Transcript yet about me, 'tis easie for me to tell you, that 'tis this: *Ii sunt frigoris effectus quales habere privatio, quæ actionis est incapax, non potest.* Vid. *Gassend. Physicam. Sect.* 1. *Lib.* 6. *Cap.* 6.

This Argument, though he begins not with it, I choose to make the first, because I think it of such weight, that, though it were the only one he could alledge, it would serve his turn and mine, since 'tis drawn from the Effects of Cold, which, though he men-

mentions them but in few and general words, experience shews to be both so manifold and so considerable, that if *Carneades* imploy an hundred times as much time to answer the Argument they afford, as I have done to recite it, he will, I think, do no more than would be necessary, and perhaps not enough to be sufficient. For, *Cold* affects the Organs of Feeling, and sometimes causes great pain in them, condenses Air and Water, and breaks Bottles that are too well stopt, congregates both Homogeneous and Heterogeneous things, increases Hunger, checks fermentation in Liquors, produces Heat by *Antiperistasis*, in deep Cellars, Mines *&c.* and yet freezes Men and Beasts to Death, dismantles whole Woods and Forrests of their Leaves, and does (I know not how many) other Feats; among which, it is not the least admirable though one of the most common, that it turns the fluid and yielding Waters of Rivers and Lakes, and sometimes of part of the Sea it self, not too far from the shoar, into firm and solid Ice, which is often in Northern Climates strong enough, not only to be travell'd upon by Merchants with their Carriages, but to be fought upon by whole Armies with their trains of Artillery. From which, and other Instances, it is manifest, that Effects so numerous and great, cannot proceed from a meer privation, or any negative thing, but require a considerable, and therefore sure a Positive, Quality to produce them.

Carneades. This Objection, *Themistius*, is, I confess, a considerable one, and of more weight than any of the rest, if not than all of them put together: But, as I think it very worthy to be answer'd,

ver'd, so I think it very possible to be well answer'd; and to give you my reasons for my so thinking, I shall distinctly consider in the Argument the two particulars which it seems to consist of.

And first we are told, that if Cold be but a Privation, it cannot be the object of sense. To clear this difficulty, which, I know, you will think it very hard, if at all possible to do, I must beg your leave to observe something about Sensation in general; not as designing an entire and solemn Discourse of that Subject, but because the particular remark I am about to make, is necessary to the Solution of our present Difficulty. I observe then, that That, which, at least in such cases as we are speaking of, produces in the mind those perceptions, which we call Sensations of outward Objects, is the Local Motion, caus'd by means of their Action upon the Outward Organs in some internal part of the Brain, to which the Nerves belonging to those Organs correspond; and the diversity of Sensations may be referr'd to the differing modifications of those internal motions of the Brain, either according to their greater or lesser Celerity, or other Circumstances, as our Friend Mr. *Boyle* has somewhere exemplify'd in the variety of Sounds; whereof some are grave, some sharp, some harmonious and pleasant, some jarring and offensive; and yet all this strange variety proceeds from the variations of those strokes or impulses, which the Air, put into motion by sonorous Bodies, gives to the ear.

To this it will be consonant, that as the Air, or rather the mind by the intervention of the Air, is differingly affected by a very grave sound, and a very acute one; though the former proceed from the

want

want of that Celerity of motion in the undulating Air, which is to be found in the latter; which slowness or imminution of motion, does, as such, participate of, or approach to, the nature of Rest: so in the sensory of Feeling, there may, upon the Contact of a Cold Body, be produc'd a very differing perception from that which is caus'd by the contact of a Hot Body; and this, though the thing perceiv'd, and by us call'd Coldness, consists but in a lesser agitation of the parts of the cold Body than of those of the hot Body, in respect of our hand or other Organs of Feeling.

And this leads me, for the farther clearing of this matter, to represent to you, that since 'tis manifest, that Bodies in motion are wont to communicate of their motion to those more slow Bodies they happen to act upon, and to lose of their own motion by this communicating of it: Since this, I say, is so, if, for Instance, a man take a piece of Ice in his hand, the agitation of the particles of the Sensory will, in good part, be communicated to the Corpuscles of the Ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the Hand losing of the motion they thus part with to the Ice, there needs nothing else to lessen the agitation they had before. And there needs no more than this slackning or Decrement of Agitation, to occasion in the mind such a new and differing perception, as men have tacitly agreed to refer to Coldness.

Eleutherius. It seems by this Discourse, *Carneades*, that you think, that Sensation is properly and ultimately made in, or by, the Mind, or discerning Faculty; which from the differing motions of the inter-

nternal parts of the Brain is excited and deter-
nin'd to differing perceptions; to some of which
Men have given the names of Heat, Cold, or other
Qualities. So that, according to you, if a consi-
erable Change or Variation be made in the most
rdinary, or in the former motion or modification
f motion of the parts of a Sensory, and consequent-
y of the parts that answer them in the Brain, new
ensations will be produc'd, whatever the cause
f this Alteration be, whether Privative or Posi-
ive.

Carneades. You do not mis-apprehend my
houghts, *Eleutherius*, and what you say gives me
rise to illustrate this matter yet a little farther
y observing, that the Sensories may be so accu-
tom'd to be affected after a certain manner by
hose external Objects, whose Operation on them
s very familiar, or perhaps almost constant, that
he Privation, or the bare Imminution of the wont-
d operation leaves the parts of the Sensory, for
vant of it, in a different disposition from what
hey formerly were in; which change in the senso-
y, if it be not too small, will be attended by a per-
eption of it in the mind. To declare and confirm
his by an example, we may consider, that though
Darkness be confessedly a Privation of Light, and
he Degrees of it, gradual Imminutions of Light;
yet the Eye, that is, the Perceptive Faculty by
he Intervention of the Eye may well enough be
aid to perceive both Light and Darkness, that is,
oth a Positive thing, and the Privation of it. And
tis obvious, that the motion of a shadow, which is
a gradual Privation of Light, is plainly, and with-
out difficulty, discoverable by the Eye; of which
the

the reason may be easily deduc'd from what I have been lately saying. And to shew you that there is on these occasions such a change made in the Organs of Seeing, as is visible even to By-standers, I shall need but to appeal to the Experiment of making in the day time a Boy or Girl look toward an enlighten'd Window, and then towards an obscure part of the Room; for when the latter come to be done, you will plainly perceive, that for want of such a degree of Light as was wont to come in at the Pupill, and straiten a little that perforation of the Uvea; that round Circular Hole, or, as you know they call it, Apple of the Eye, will grow very manifestly larger than it was before and than it will appear again, if the Eye be expos'd to a less shade Light.

This observation may be seconded, by what happens to a man, when coming out of the Sun-shine where the Sun-beams much contract his Pupill to shut out an excessive Light that would be offensive to the Organ, he comes presently into a dark room where he must continue some time before he can see others as well as he is seen by them, whose Pupills have had time to be so inlarged, as in that darker place to let in light enough to make Objects visible to their Eyes, which are not so to his, whose Pupills are yet contracted by the Light they were but just before exposed to. To this I might add divers other *Phænomena*, explicable upon the same grounds; but I shall rather chuse to relate to you an uncommon Accident, which happening to eyes somewhat unusually disposed, do's more remarkably discover, what alteration Darkness, or a privation of Light, may have upon those Organs. I know

a

very Learned man, who is no less studious of Mathematicks, and other real parts of Knowledge, than skill'd in those which are taught of the Schools: This *Virtuoso*, who seem'd to me to have something peculiar in his eyes, confess'd and complain'd to me, that if he come, though but out of a moderate light of the open air, into a room that is any thing dark, he does not only feel such an alteration as other men are wont to do on the like occasion; but is so powerfully affected by it, that he thinks, he sees flashes of fire before his Eyes, and feels a troublesome discomposure in those parts, that sometimes lasts an hour or two together, if he so long continue there.

Eleutherius. I know not, *Carneades*, whether after this, you will think it any great confirmation of your Opinion, that *Aristotle* has somewhere his saying, that, *Oculus cognoscit Lucem & Tenebras.*

Carneades. I thank you, *Eleutherius*, for so pertinent an Allegation, though not for my own sake, yet for theirs that will more easily receive a Truth upon the Testimony of *Aristotle*, than that of *Nature.* And now, I hope, that *Themistius* will consent, that dismissing the Argument hitherto examin'd, we proceed to the next.

SECT. III.

Them. SInce you will have it so, I shall comply at present, and the rather, because not only I foresee there will be occasion to speak of

of it again, but because you Experimental Philoſophers, that are wont ſo much to cry up the Informations you think you receive from Senſe, ſometimes in ſpite of contrary dictates of Reaſon, will I hope, be prevailed with by the Argument I am about to propoſe, which is ſo manifeſtly grounded upon Senſe, that without denying that we do feel what we feel, we cannot deny Cold to be a Poſitive Quality. For thus *Gaſſendus* moſt convincingly argues; *Cùm per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea ſentitur non poteſt dici mera privatio, aliúdque prorſus eſſe apparet ſentiri aquam frigidam, & ſentiri non calidam. Et fac eandem aquam gelari, ſentietur haud dubiè frigidior, an dices hoc eſſe nihil aliud quàm minùs calidam ſentiri? Atqui calida jam antea non erat, quomodo ergo potuit minùs calida effici?*

Carnead. I will not ſay, *Themiſtius*, his Argument is not ſpecious, but you, perhaps, or at leaſt *Eleutherius*, will not affirm it to be more than ſpecious, if you pleaſe to conſider with me two or three things that I have to ſuggeſt about it.

And firſt, to ſhew, *Themiſtius*, that, whatever he was juſt now intimating, Experimental Philoſophers do not prefer the immediate Impreſſions made on the Senſes to the dictates of Reaſon though they think the Teſtimony of the Senſes however ſometimes fallacious, much more informing than the Dictates of *Ariſtotle*, which are oftentimes (and that groundleſly) repugnant to them; I will repreſent to you, that the Organs of Senſe conſider'd preciſely as ſuch, do onely receive Impreſſions from outward Objects, but not perceive

what

what is the cause and manner of these Impressions, he Perception properly so called of Causes belonging to a superior Faculty, whose property it is o judge whence the alterations made in the Sensories do proceed, as may easily be proved, if I ad time and need to do so, by many Instances, wherein the Senses do, to speak in the usual phrase, nis-inform, and, as far as in them lies, delude us, nd therefore must be rectified by Reason. As hen the Eye represents a straight Stick, that has art of it under water, as if it were crooked; and two ingers laid cross over one another, represent us single Bullet or a Button roll'd between them, as there were a couple: So that 'tis very possible for I forbear saying 'tis true, having not yet proed it,) that though the Sensory be very manifestly nd vehemently affected upon the contact of cold Vater, or other cold Bodies, yet the cause of that npression or affection is, and may be judged and etermin'd by Reason to be, other, than that which he Sense may to an inconsiderate person suggest. s when a Child, or one that never heard of the hing before, first sees a Stick, whereof one part is n the Air, and the other under Water, he will resently, but erroneously, conclude that *Phæ-nomenon* to be caused by the Stick's being crooked r broken.

Next we may consider, that Sensations may in ivers cases be made, as well from alterations that ay happen in the internal parts of the Body, as om those that are manifestly produced in the exrnal Organ, by external Objects and Agents; s may appear by Hunger, Thirst, the Titillation f some parts of the Body, barely upon Venereal

thoughts, and (which belongs directly to our pre-
sent Argument) the great Coldness that we have
known Hysterical Women complain of in their
Heads and Backs, and the great and troublesome
degree of Cold, which we every day observe upon
the first invasion of the Fits of Agues, especially
Quartans; which troublesome symptomes, that
sometimes last for several hours, are therefore com-
monly called the Cold Fits.

And now it would be seasonable for me to call
upon you to remember (and add to what I have
now said) that which at the beginning of our con-
ference I took notice to you of about Sensation in
general; if I did not presume that those things are
yet fresh enough in your memory, to allow me to
proceed directly to answer the Objection, which
I shall do, though not like a *School-man*, yet like a
Naturalist, by giving an account of the propose
Phænomenon, without having recourse to that *Hypo-
thesis* which 'tis urged to evince.

I observe then, that though in the respective
sence above-mention'd, Water, wherein the Ob-
jection supposes the hand to be plunged, be cold
in regard its parts are less agitated, than the Spirits
and Bloud harbour'd in the Hand; yet in a Philo-
sophical sence, it is not quite destitute of Heat,
since 'tis yet Water, not Ice, and would not be
Liquor, but by reason of that various agitation of
its minute parts, wherein fluidity, a Quality essen-
tial to Liquors, consists. Upon the score of the
respective Coldness of the Water, the Hand is
refrigerated; for the Spirits and Juyces of the
Organ meeting in the Water with Particles much
less agitated than they are, communicate to them
some

some part of their own Agitation, and thereby lose it themselves, upon which Decrement of wonted Agitation, such a change is made in the Sensory, and, (though not so manifestly) in some other parts of the Body, as is perceived by the Animadversive Faculty under the Notion of Coldness; Sensation, (whatever obscure Definitions are wont to be given of it) being indeed an Internal Perception of the changes that happen in the Sensories.

And if now, as the Objection supposes, the Water wherein the hand is plunged comes to be more refrigerated than before, the Spirits, Blood, and other parts of the hand, finding the Aqueous Corpuscles more slowly moved than formerly, must, according to the Laws of Motion, (according to which a Body that meets another much more slowly moved than it self, communicates to it more of its motion than if 'twere less slowly moved,) transfer to them a greater measure of their own motion, and consequently themselves come to be deprived of it: And upon this increase of the slowness of motion in the parts of the hand, there follows a new and proportionable perception of the Mind, and so, a more vehement sensation of Cold. But though it be not to be admired, that the bare slowness of motion in the Object should be discernible by Sense, albeit it seems to participate of Rest, which with you passes for a Privation, since the Ear perceives when a Voice grows faint, and when a sharp Sound degenerates into a flat one; and we can perceive by the hand (abstracting from Heat and Cold) the celerity or slowness of Bodies that in their passage strike upon it, as for instance, of

Winds or streams; yet this is not the only thing I think fit to be taken notice of on this Occasion. For, I consider farther, that besides the most consistent and stable parts of the Hand, there are from the Heart and the Brain fresh blood and spirits continually transmitted to the Hand; and the former of these, the Blood, is, according to the Laws of its Circulation, and after it has received a great change in the much refrigerated Hand, carried back through other parts to the Heart; whence it is in the same Circulation distributed to the whole Body. To which may be added, that when the great refrigeration of the Hand happens, external Agents may contribute to the Effects of it, as I shall by and by have occasion to shew.

If then you please to remember, that upon the turning ones eye to the dark part of a room less inlighten'd than the Window, though Darkness be but a Privation, and though the Obscurity of that part be not absolute, but consist only in a less degree of Light; yet the action of the Spirits and other parts of the Body is so changed upon occasion of the Light's acting more faintly than was usual upon the Organ, that the Pupill is immediately and manifestly dilated, and in some cases, as in that which I mention'd to you of a Learned Man, much considerabler Effects ensue; you will not wonder, that, where not only the Spirits, but the Blood, (whence those Spirits are generated) that circulates through the whole Body, and upon whose Disposition all the other parts so much depend, is very much disaffected, there should be felt a great alteration in the Hand, which is the most immediately expos'd to the action of the cold Water.

And

And for the Reasons newly given, it ought to be as little strange, that in other parts of the Body, the disorder'd and not circulating Blood should have its wonted action on them considerably alter'd; since the more stable parts, and especially those external ones that are most expos'd to the Cold, have their pores straiten'd, and consequently their Texture somewhat alter'd; on the same occasion on which the wonted agitation of the Spirits with the Particles that compose the Blood, is notably lessen'd. And that such Causes may produce great Effects in a Humane Body, you will be more prone to admit, if you consider the disorders that happen in the cold fit of an Ague, and oftentimes upon the shutting up of those excrementitious steams that are wont to be discharged by insensible Transpiration; to whose being stop'd in the Body by the constriction of the Pores, which chiefly happens through Cold, some Learned Physicians, especially the famous *Sennertus*, impute the cause of most Feavers, as indeed Experience it self does but too frequently shew it to be guilty of many.

Philoponus. I confess, *Carneades*, you have said some things that I thought not on before; but yet *Gassendus*'s Argument seems to be such, that I fear 'twill be hard to hinder many from saying, That if Cold be but a Privation of Heat, 'tis a Privation of a strange nature: For, it may be introduc'd into Bodies that were not Hot before, nay, in some cases, into such as are naturally Cold, and also by consequence must have been put into a preternatural state to be at any time Hot.

Carneades. This Objection, *Philoponus*, being in

effect so much the same with that of *Gassendus*, that it differs from it but in the dress you give it, 'twill scarce require a peculiar and distinct answer; and therefore, as soon as I have reminded you of the Distinction, that we have formerly made of the Vulgar and Philosophical sence of the word *COLD*, I shall need to alter but a little what I said before, by telling you, that since Fluidity consists in the various agitation of the insensible Corpuscles of a Liquor, and that Heat consists in a *tumultuary*, but a more vehement agitation of the insensible parts of a Body, and so, that Hot Water scarce differs otherwise than gradually, from that which is cold to Sense; if Cold be taken in the larger and Philosophical sence, it may well be said, that as long as Water retains the form of Water, and so continues to be a fluid Body, though it may be very cold to the Touch, yet it is not absolutely or perfectly cold, and therefore is capable of a farther degree of coldness, which it receives when brought to Congelation: for till then it was not destitute of those agile Corpuscles, that were requisite to keep it fluid; and till then, *Gassendus* himself must acknowledge, that it was not absolutely or perfectly cold; because He, as you may remember, did in his former (but lately mention'd) Argument ascribe the Glaciation of Water to the invasion of those that he calls Corpuscles of Cold.

Eleutherius. Give me leave to add, *Carneades*, that 'tis not every Glaciation it self that brings Liquors to be perfectly Cold in the Philosophical sence of that expression, and quite expells or subdues all the agile Particles that were in the Water before 'twas turn'd into Ice. For, I think, that to

effect

effect this change, 'tis sufficient, that so many of these restless Particles be destroyed or disabled, that there remains not enough of them to keep the Water in a state of Fluidity, so that the surplusage may yet continue in the frozen Liquor, and whilst they are there, perform several things, as the making it evaporable in the Air, and even odorous, and by *their* recess or destruction the Ice may grow yet more cold. And *as* this Notion suits very well with the differing degrees of hardness, that we find in differing portions of Ice, sometimes upon the account of the matter, (as frozen Water is harder than frozen Oyl,) and sometimes upon that of the different degrees of Cold in the same Water or other matter, (as our Friend somewhere observes;) so it may be highly confirmed by an Experiment I saw him make, but that is not yet published.

The summ of the Experiment was this; That he first put an Hermetically seal'd Thermoscope into a Glass broader at the top than at the bottom, and greas'd the inside with Tallow, that Ice might not strongly stick to it. In this Glass was put Water, more than enough to cover the ball of the Instrument; and that Water being warily frozen, notice was taken, whereabouts the tincted Spirit of Wine rested in the Stemm; after which, the Instrument and the Ice being removed into the open Air, upon an exceeding frosty morning, the Ice was taken off from the ball, and presently after, the tincted Liquor, as the maker of the tryal expected, subsided a pretty way (the length of the Instrument considered) below the former mark; which argued that he rightly guess'd, that such a degree of Cold as is sufficient to turn Water into Ice, may not produce

a Body perfectly Cold; this Ice it self keeping the inclos'd ball, in a sence, warm, by fencing off the Air, which, at that time, (even in our temperate Clime) by the Effect appear'd to be colder than the very Ice. And, me thinks, it may strengthen *Carneades*'s Discourse, to represent, that there is no sufficient cause, why many things that are reckon'd among Privations or Negations by the Peripateticks themselves, as well as Cold is by *Carneades*, may not admit of degrees; as may be exemplified by Deafness, Ignorance, and divers other things. And to bring a case, not very unlike that under consideration, we may take notice of a total Eclipse of the Moon, which you know alwayes happens when she is at the full. For Darkness in the Air being acknowledged to be a Privation or Negation of Light, when the Earth interposed between the Moon and the Sun has Eclipsed her, for instance, nine digits, (as Astronomers speak,) Men generally complain of darkness in the air, though there remain a considerable part of the *Discus* or the Hemisphere of the Moon obverted to us yet inlighten'd by the Sun; but when the interpos'd Earth proceeds to cover the remaining three digits, and so makes the Eclipse total, the darkness also is said and esteem'd to be much increas'd: Nor would men otherwise be perswaded, though *Themistius* should tell them, that the Air cannot have grown darker, though it were dark before, and indeed though the Air was more and more darken'd in proportion to the increase of the Eclipse, yet it was never compleatly darken'd 'till it became total. But I fear I dwell too long upon one Argument.

SECT.

SECT. IV.

Eleu. LEt me therefore, *Carneades*, ſumm up what I take to be your Doctrine, and tell theſe Gentlemen, that I think you do not look upon the Senſation of Cold as a thing effected by an intire Privation properly ſo called and conſider'd as ſuch, but that according to you that ſlowneſs of motion in the Particles of cold Water, which the Hand finds when 'tis thruſt into that Liquor, does occaſion the Spirits and the Corpuſcles of the Blood to part with to thoſe of the Water a conſiderable ſhare of their own ſurpluſage of agitation, whereby they loſe it themſelves, upon which is conſequent a Perception of this change made in the Hand, which, if it be very great, is alſo frequently accompanied with ſome ſenſible change in other parts of the Body, occaſion'd chiefly by the frequent returns of the circulating and highly refrigerated Blood to the Heart, whence 'tis diſperſed to the whole Body. According to which Doctrine, the Senſation of Cold is but a perception of the leſſen'd Agitation of the parts of the Hand either ſtable or fluid, eſpecially of the Blood; which alterations are in great part produced, not by the coldneſs of the Water, as Cold is a Privation, but from the new modification of the action of the Blood and Spirits upon the Nervous and Membranous parts, the conſtriction of whoſe Pores concurrs to that Modification. And, if I do not miſunderſtand your Opinion, *Carneades*, methinks it may be confirmed by this which I have known obſerved

ſerved by experienc'd Chirurgeons, that by too ſtrict Ligatures unskilfully made, an Arm, for inſtance, may be gangrenated; in which caſe, all the proper and immediate effect of the Ligature is but the conſtriction of the part, though that conſtriction being unuſual and exceſſive, it proves the occaſion of the mortifying of the Hand and Arm by hindring the free and uſual acceſs of the Blood and Spirits to that Limb; upon which, by the depraved action of the parts of the Body one upon another and the concurrence of external Agents, there enſues a Mortification or Gangrene of the part which, if due Remedies be not timely employed, is communicated to other parts and kills the Man.

Carneades. Whatever become of your Inſtance *Eleutherius*, I thank you for your readineſs to propoſe it in favour of my Hypotheſis, which you will eaſily judge not to be much concern'd in the cloſe of the excellent *Gaſſendus* his Arguments for the Poſitive Nature of Cold. For though theſe words of his ———

Themiſtius. You may ſave your ſelf the trouble of naming them now, ſince, whatever they may ſeem to you, I profeſs I look upon them as containing a diſtinct Argument, which I ſhall therefore propoſe in its due place hereafter; but in the mean time, and before we leave the Argument you would have us diſmiſs, give me leave to remind you, *Carneades*, of ſome part of your former Diſcourſe, and to take thence a riſe to tell you, that you, who told us that we ought not to conſider the Operations that Qualities have upon our own Senſories only, but alſo what they do to other Bodies.

will

ill, I hope, allow me to demand, how a Privation, r if you will, how an Imminution of Motion can roduce the hundredth part of those Effects which e daily see produc'd by Cold in the Bodies that e about us.

Carneades. I thought, *Themistius*, I had intima- d to you already, what might have prevented our Question; but since I see 'tis otherwise, you hall not find me backward to explain my self a little ore fully. I do not pretend, that either an abso- te privation of motion in a Body, or a slowness f motion in the parts of it, is, as such, the proper fficient cause of the Effects, vulgarly but unduely crib'd to Cold alone; for, in my opinion, Cold rather the Occasion, than the true Efficient Cause f such Effects, which, I think, are properly to be cribed to those Physical Agents, whose actions or perations happen to be otherwise modified than ise they would have been upon the occasion of that nminution or slackness of Agitation which they neet with in cold Bodies, by occasion of which ney are both deprived themselves of the Agitation ney communicate to such slow Bodies, and there- y act no longer as, were it not for that loss, they ould, and by a natural consequence of this change, hich is made in themselves, they do also, though ss notably, modifie the action of other Bodies up- n them: From which unusual alterations happen- g in a World so fram'd as this of ours is, and go- ern'd by such Laws respecting Motion and Rest as re observed among Bodies, there must in all pro- ability result many new, and some of them consi- erable, *Phænomena*. For though Quiescent Bodies eem not to have any action which among corporeal sub-

substances seems to be perform'd only by Loc motion; yet Bodies quiescent themselves may con curr to great Effects both by determining the mo tions of other Bodies this or that way, or by rece ving their motion totally or in part, and so depri ving the formerly moving Bodies of it: Thus th Arches of a Bridge, though immoveable themselves by guiding the water of the River that beats again them, may occasion a rapid and boisterous stream capable to drive the greatest Mills, and perform more considerable effects, though the River, be fore it met with them, ran calmly enough, as i evident at *London* Bridge, especially when the Wa ter is near a low Ebb. And now I have mention'd Water, I will add, that though Water it self be no a quiescent Body, but being a Liquor has its part in perpetual motion among themselves; yet sinc that agitation is exceeding slow in comparison of th swiftness of a Cannon-bullet, in respect whereo the calm surface of the Water participates of th nature of a Quiescent Body, Bullets themselve shot from out of Guns elevated but little above th Level of the Water, (upon which score they make but a very sharp angle with it;) these Bullets, I say, do not unfrequently rebound from the Surface of the Water, and consequently, even these so wonderfully swift Bodies receive a new Determination from it.

Eleutherius. One may add, *Carneades*, to your Instances, that in a Tennis-Court the wall, against which Balls are strongly impell'd by a Racket, contributes much to the mischief that those Balls do often to By-standers in the Gallery, as the Wall, though it self unmoy'd, gives a new Determination

to

the moving Ball, and by its resistance makes it bound or reflect at an Angle equal to that of the alls incidence. And this concurrence of the Vall to such Effects is the more evident, because this other circumstance, (which also befriends our Opinion,) that, if the impell'd Ball, instead of tting against the Wall, hits against the Net, this yielding deprives the Ball of its *Impetus*, and hin-rs the reflection that would else ensue.

Carnead. You have, I confess, somewhat pre-nted me, *Eleutherius*; but yet not altogether: or though I was going to propose the example of Ball, yet 'twas in somewhat a differing way; for was about to propose to *Themistius* the example a Ball, which if it be forcibly and perpendicularly rown against the hard Ground, has its Determi-ttion so alter'd, that whereas it moved before wards the Centre of the Earth, it immediately, ith almost the like swiftness of motion, tends di-ctly upwards. And if on the other side you row the Ball, not against a hard, but against a uddy piece of ground, it will not rebound, lose-g its own motion, by communicating it to the rts of the yielding Mudd; as may be in some mea-re illustrated by the great commotion made in a all Pond of Water, when a Ball (or a round one) being but gently let fall upon the surface of , has its motion thereby deaded, and transferr'd the parts of the Liquor, which perhaps will e visibly agitated at the remotest brink of the ond.

Eleutherius. These Examples may conduce much explicate your Doctrine, *Carneades*, but since *Themistius* himself was so equitable a while agoe, as

to

to allow you much time to defend such a Parado as yours against *Gassendus*'s Argument, I shall wit your leave (of which I doubt not) to the Exam ples already mention'd add this one more. Sup pose upon a stream that runs through some Tow (which is not very rare) there were built a num ber of differing Mills, some for the grinding Corn, others for the Fulling of Cloth, others fo the moving of Bellows to melt Oars and Metals others for forging of Sword-blades; others for ma king of Paper, and others for other uses: And sup pose that an Enemy coming to besiege this Tow should successfully imitate *Cyrus*'s Stratagem, whe by suddenly diverting the course of *Euphrates* h took *Babylon*; would it not be consequent to th derivation of the Water into some lower plac and this ceasing of the Stream to run in its forme Channel, that the action of all these Mills, by whic so many differing operations were perform'd, mu of necessity cease too? though the Besiegers do no produce this change by any positive and direct vio lence that they offer to the Mills, but onely b hindring them from receiving the wonted Impuls which were requisite to keep them in motion.

Carneades. I dislike not your Instance, *Eleuthe rius*, which yet will not altogether render usele what I was going to say about a Wind-mill, whic will illustrate one part of my Doctrine, for whic your Water-mill does not seem to have been in tended. And that this Example may the better d so, I will suppose a Wind-mill to be built in som low place near the bank of your stream, whic stream we will suppose to be lyable, as some other are, upon the falling of great and sudden rains up

n the neighbouring hills, to overflow its banks, in ıse the increase of the Water be not then hindred y the Wind-mills lifting up constantly some parts it, and conveying it away by Pipes or otherwise: nd then let us suppose, what really sometimes ppens, that the Wind should so cease, that there ould not blow any wind strong enough to move e sails for a great while together; will it not ence manifestly follow, that by reason of this abnce of the Wind, which absence has the nature of Privation or Negation of a Stream-like motion in e Air, not only there will be a ceasing of those ffects and Operations whatever they were, that ere wont to be perform'd within the Mill it self, t also there will be a durable intermission of that ain work of the Mill whereby it carried off such quantity of Water; which work ceasing with the Vind, whilst the flowing in of the Water does not ease too, but continues as formerly, the still-inreasing Water must bear down or overflow its ronted Banks or other Boundaries, and by its unly effusions drown the neighbouring parts, and roduce the Disorders, that is, the new *Phænomena*, aturally consequent to an Inundation made by such quantity of Water. And if the Water conveyed way by means of the Mill through Pipes or Chanels were employed to water Grounds, or other articular uses, the growth or fertility at least of he Vegetables that Water was requisite to nouish, or the other uses to which it was necessary, must consequently be much, if not totally, hinred.

Philoponus. I know not whether we may not efer to the Subject of your Discourse, what may

be

be observ'd in *Paralytick* affections, where a little Viscous or Narcotick Humour obstructing or otherwise disaffecting one part of a Nerve, though its proper and immediate action be only to hinder or weaken the Spirits, that were wont, in competent plenty, to pass freely along the Nerve to the Muscle whereto it leads; yet the action of the other parts of the Body and the Relaxation of the Fibres do oftentimes produce a tremulous motion in the Limbs, and particularly the Hands; and sometimes also the Mouth, Neck and other parts, are drawn awry in an odd and frightful manner.

Carneades. Though I approve of *Philoponus*'s fancy, yet I think a more quick and notable Instance to the same purpose may be taken, from what happens to Birds, and Rats, and Cats, and such kind of warm Animals, in Mr. *Boyle*'s Engine. For *as* the Air by the agitation of its parts, or that of some Ethereal substance that pervades it, entertains the fluidity of Water and other Aqueous Liquors; and when that agitation is hinder'd or too much lessen'd, Water ceases to be fluid, and upon that divers Violent Effects ensue, wont to be ascrib'd to Glaciation: *so* the bodies of warme Animals, having been born in the Air, and perpetually expos'd to the action of it, (though that be seldome heeded) when being plac'd in the Receiver of the Air-pump, and by the operation of that Instrument, which withdraws the former Air and keeps out the new, the Air that was wont continually to act upon them, is kept from doing so any longer, though this absence, or not touching of the Air, be but a privative or negative thing, yet by reason of the structure of the *Animal*, his Spirits

ts and Humours, assisted by the concourse of more eneral Causes, are brought to act so differingly om what they were wont to do, that the Blood nd Juyces swell, the Stomach vomits, the *Animal* rows faint and staggers, the Limbs, and at length he whole Body are convulsed, the Circulation is opp'd, and at last the whole *Animal* kill'd; and l this done in a very few minutes of an hour, ithout the visible intervention of any positive A- ent.

Eleutherius. What you say, *Carneades*, concern- g the quick and violent Death of warm *Animals* n Mr. *Boyle*'s *Engine*, puts me in mind of an Ex- eriment I saw made in that Instrument upon cold *nimals*, which, methinks, may well illustrate the omparison we lately employed of a Wind-mill. or *as* those great artificial Engines lose their Mo- on, and the Operations depending on it, if that tream of Air, we call the Wind, be held from eeping them going; *so* Insects and some other old *Animals* have their differing motions so de- endent upon the contact of the Air, that, as soon s ever they are deprived of it (by the Engine we re speaking of) divers sorts of them will lye ovelesse as if they were dead; and I have known veral of them that were put in together, continue n that state for many hours, as long as it pleas'd ur Friend to with-hold the Air; but when once e thought fit to let a Stream of Air enter the eceiver, these seemingly dead Animals, as Worms, ees, Flyes, *&c.* like so many little Wind-mills of Nature's (or rather her great Author's) making, ere set a moving in various manners (as creeping, ying, *&c.*) suitable to their differing *Species*.

Carneades. So that to summ up in a few words the Result of these Instances, and the rest of the past Discourse on the same Subject, it appears by what has been said, that the Effects undeservedly ascrib'd to Cold, need not in our Hypothesis be referr'd to a Privation, but to those positive Agents or active Causes, which by their own nature are determin'd to act otherwise on, or suffer otherwise from, one another, in cases, where there is a great hindrance or ceasing of wonted agitation, than where there is not.

SECT. V.

Themist. IT may perhaps now be time to put *Carneades* in mind, that, in what he has been discoursing all this while, he has propos'd Answers but to a couple of *Gassendus*'s Arguments, and left the rest untouch'd.

Carneades. I should readily grant, *Themistius*, that I have dwelt too long upon so few Arguments, if I did not hope, that by fully answering Them and giving the Company a particular account of my Notions concerning Cold, I might very much shorten and facilitate the remaining part of my Task, which engages me to return Answers to the other Arguments you speak of, the grounds of solving which, I think, I have already laid in the past Discourse. And therefore you may go on to propose the next Argument of *Gassendus*, as soon as you please.

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Themistius. And I shall do it, *Carneades*, in that Learned Man's own words, which I well remember to be these: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quamobrem manus intra istam, non intra illam refrigeratur? an quia calor manûs intra frigidam retrabitur, manúsque proinde relinquitur calida minús? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? At si frigus est tantùm privatio, quidnam calor ab illa metuit? Privatio sanè nihil est, atque adeò nihil agere, unde ejus motus incutiatur, potest.* Gassend. *Lib.* 6. *Cap.* 6.

Carneades. This Objection, *Themistius*, may indeed puzzle many School-Philosophers, but will easily admit an answer in my Hypothesis. For that does not oblige, or so much as tempt, *me* to ascribe (as a *Peripatetick* would do,) to a meer Quality, (for such is Heat,) both a knowledge of its danger, and a care and skill to preserve it self from its Enemy, the Cold, by a retreat inwards. For, agreeably to what I lately delivered, 'tis obvious for me to explicate the *Phænomenon* thus: When a man puts his Hand into warm Water, the agitation of the Corpuscles of that Liquor surpassing that of the Spirits, Blood, and other parts of his Hand, cannot but excite in him a sense of Heat; but when he puts the same Hand into cold Water, the case ought to be much altered, not by any imaginary retreat of the Spirits, but the communication of motion by other parts to the surrounding Water, by which means there must be in the Hand a great lessening of the former agitation of its parts, the perception or sense of which decrement of motion is that which we call the Feeling of Cold.

Eleutherius. I think indeed, *Carneades*, that though this Argument may be considerable against those that the Learned framer of it might have in his Eye, it is but invalid against you. But can you as well decline the force of that other Objection, which *Gassendus* more insists on, and which seems as directly to oppose *you* as any other Adversaries of his Hypothesis?

Themistius. I presume, *Eleutherius*, you mean that cogent Argument, which *Gassendus* proposes and prosecutes more fully than the rest, deducing it from the way of artificially freezing Water by a mixture of Snow and Salt, placed about the outside of the Glass that contains the Liquor. For from this practice he rationally concludes, that since this frigorifick mixture is through the Glass able to freez the Water into Ice, it may as justly be affirm'd to act by Corpuscles of Cold, as Fire can be to act by Calorifick Corpuscles, when kindled Coals, placed on the outside of the Glass, make the contained Water boyl. And this cogent Argument will, I hope, prove the more satisfactory to *Carneades*, since 'tis not drawn from what he would call a disputable Peripatetick Notion, but from the same Quiver, whence he affects to take his Shafts, *Experience* it self.

Carneades. I freely acknowledge, Gentlemen, this Argument to be very plausible; but that it is clear and cogent, I must not grant, till I be better satisfied that it is so.

And, I shall scarce think it as evident, that Ice and Salt act by a Positive Quality, as that burning Coals do so, though Cold seems as well to be produc'd by the former, as Heat by the latter. For in-

innumerable Experiments shew, that *Heat*, in the Fire especially, is a Positive Quality, consisting in a tumultuary and vehement agitation of the minute parts of the Body that is said to be hot, and producing also in the Bodies that 'tis communicated to, a local motion, which is manifestly a positive thing. This is so evident, in the heating of Bodies by mere attrition, the smoaking and melting of divers Bodies in the Sun-beams (especially at fit times of the day and year,) the sudden boiling and dissipation of Water, Oil, *&c.* dropt on a red-hot iron, and many other obvious instances, that 'twere a needless work to go about to prove it, especially since both *Themistius*'s Peripateticks, and *Gassendus* himself, who so often disagree about other things, agree in confessing that Heat is a Positive Quality.

Themistius. But remember, *Carneades*, that the grounds on which they do so, are the same, on which *Gassendus* justly builds the Proposition, that *Cold* also is a Positive Quality.

Carneades. I did not forget that, *Themistius*; for I was about to subjoyn to what I last said, that 'tis evident not onely by the confession of my Adversaries, but by that (which to me is much more considerable) of Nature her self, proclaiming it in the Instances I just now mentioned, that Heat is a Positive Quality; whereas that Cold likewise is so, does not appear to me by the Experiment of Artificial Congelations. For, in this all that is clear in matter of fact is, that Snow or beaten Ice and Salt are put about a Vessel full of Water or other Aqueous Liquor, and that, within a while after, this Water begins to be turn'd into Ice; but, that this Glaciation is perform'd by swarms of atoms of Cold,

that permeating the Glass, invade and harden the Liquor, is not perceiv'd by Sense, but concluded by a Ratiocination, the cogency of which I am allow'd to examine without affronting the certainty of Sense; *that* not being concerned in the case. If then an intelligible way can be proposed of fairly explicating the *Phænomenon*, besides that insisted on by *Gassendus*, the objection drawn from this Experiment against my Hypothesis will be invalid. And such an Explication Monsieur *Des-Cartes* ingeniously gives in his Meteors: *Quia Materia Subtilis* (sayes he) *partibus hujus aquæ circumfusa crassior aut minus subtilis, & consequenter plus virium habens, quàm illa quæ circa nivis partes hærebat, locum illius occupat, dum partes nivis liquescendo partibus Salis circumvolvuntur. Facilius enim per salsæ aquæ quàm per dulcis poros movetur, & perpetuo ex corpore uno in aliud transire nititur, ut ad ea loca perveniat in quibus motui suo minùs resistitur: quo ipso materia subtilior ex nive in aquam penetrat, ut egredienti succedat, & quum non satis valida sit, ad continuandam agitationem hujus aquæ, illam concrescere sinit.*

Lib. Meteor. Cap. 3.

Philoponus. I leave *Themistius* to consider, whether this Explication be without Exception; but I confess it is not without Analogy, and that even amongst the four first Qualities themselves. For when we *Chymists* have a mind to dry (for instance) the *Calces* or Precipitates or other Powders, from which we have filtrated the Liquors we employ to wash or dulcifie them, 'tis usual either to put the Filters, wherein these Powders remain almost in the form of Mudd, or to spread the stuff it self upon brown Paper or pieces of Brick or Chalk,

Chalk, which much haſten the exſiccation of the things laid upon them, not by any drying Particles which they emitt into the ſoft ſubſtances, but by imbibing the ſuperfluous parts of the Liquor, and thereby freeing from them the Subſtances to be dryed. And, I remember, I have ſeen our Friend Mr. *Boyle*, by immerſing a piece of ſoft crumb of bread into an actually cold Liquor, that would hatily imbibe its Aqueous Corpuſcles, and dry it in a minute or two of an hour ſo as to make it feel hard.

Eleutherius. Theſe inſtances bring into my mind another Chymical Experiment, that I have ſeen made by the ſame Gentleman, which was; That by putting into weak Spirit of Wine a ſufficient quantity of Salt of Tartar, he quickly defleamed the Spirit without Diſtillation, or ſo much as Heat. And this will the better illuſtrate the *Carteſian* Explication, becauſe 'tis manifeſt by the change that will be made of the moſt part of the Salt of Tartar into a Liquor that will not mix with the now defleamed Spirit of Wine, that the reaſon of the Operation is, that the Aqueous Particles of the Phlegmatick Spirit, finding, it ſeems, more convenience or facility to continue their motion among the Fixt Corpuſcles of the Salt, than the Vinous ones of the Spirit, paſs into the Alkaly and diſſolve it; and thereby deſert the Liquor through which they were diffuſed before. And I know another Saline body, that ſo unites with Water, as not to be, by the Eye, diſtinguiſhable from it, and yet is of ſuch a Texture, that Water is ſo much leſs diſpoſed to mingle with it than with Spirit of Wine it ſelf, that it will forſake the Body it kept in agitation,

to pass into this Spirit; and so leave that which it kept in the form of a Liquor before, to appear in the form of a consistent Body; which instance comes somewhat nearer than the former to the Experiment of Glaciation.

Carneades. Though what you have recited, Gentlemen, be not unwelcome to me, yet, I think, I can propose you an Experiment fitter to dilucidate the *Cartesian* Explication. For, I remember, that our common Friend, having a mind to shew, that a small proportion of agile matter, invisibly diffus'd through a Body that would be otherwise consistent, may bring it to, and keep it in, the state of Fluidity; devised and shewed me the following Experiment. He took Camphire broken into small bits, and casting a convenient quantity of it upon *Aqua fortis*, suffer'd it to float there, till without Heat the Camphire was dissolv'd into a Liquor, and it look'd and felt like an Oyl, which, though shaken with the *Aqua fortis*, would emerge to the top again. If this Oyl were kept well stopt, that the Spirits of the Menstruum might not evaporate, it would (as he affirm'd tryal had taught him) continue long fluid, he having sometimes kept it a year or two or more. And that 'tis the agile Spirits of the *Aqua fortis* that keep the Camphire fluid, he has made probable by divers things that I must not now stay to recite. And that the quantity of these agile Particles is but small, I am induc'd to think by this, among other things, that when I have made a small parcel of but moderate *Aqua fortis* turn a pretty proportion of Camphire into Oyl, & separated that Oyl from it, I could, by casting fresh Camphire on the same *Menstruum*, reduce that also into the form of

Oyl.

Oyl. Now, that these Fluidifick Spirits (if I may so call them) are not sensibly warm (no more than the *Cartesian Materia Cœlestis*) in Water, is manifest to the Touch: And whereas I at first suspected, that the reason, why the pouring of this Oil into Water doth presently reduce it into Camphire again, might be the coldness of the Water; I after thought, upon a farther information, that the reason rather was, that the Nitrous Spirits being dispos'd to pass out of the Oyl into the Water, this Liquor readily imbib'd and diluted them, and consequently disabled so many of them, that those that remain'd could not do their former work any longer: since he had tryed purposely, that the Reduction of the Oyl into Camphire would presently be made, though that Liquor were not pour'd into Cold Water but Hot; so that the agitation, that it received from the particles of the *Menstruum*, though not to our Touch sensibly warm, was much more efficacious, than that which it received from the Heat of the Water.

Eleutherius. I know not, whether besides the Instances that have been now proposed, one may not alledge such an Argument also in favour of the *Cartesian* opinion about Cold, as would not be insignificant, though it should be made appear, that Cold may sometimes be produced by or upon the Emission of Corpuscles, that in some sence may be call'd Frigorifick. For there may be Corpuscles of such a Nature, as to size, shape, and other attributes, as to be fit to enter the Pores, and pierce even into the inward parts of Water, and some other Bodies, so as to expell the calorifick Corpuscles they chance to meet with, or to clog or hinder their activity, or on some other account consi-

de-

derably to lessen that agitation of the minute parts by which the Fluidity of Liquors and the Warmth of other Bodies is maintain'd. But, even in such cases, though the Agent and the Actions that produce Coldness, be Positive things; yet the Nature of Coldness it self may consist in a Privation. As when a man is kill'd by a bullet, his Death is effected by a Positive and even impetuous Action, and yet Death it self is but a Privation of Life. If also in a dark Room a man cast cold Water upon a burning Coal, though the Water act by its Positive Quality of moisture, and, by virtue of that, extinguish the Fire, and by that means destroy the Light, yet the Darkness that is consequent upon this action is not a Positive thing but a Privation.

SECT. VI.

Philop. THe pause you here made, Gentlemen, makes me think it seasonable to put the Company in mind, that it begins to grow late, and therefore to call upon *Themistius* to produce what he has yet to alledge out of *Gassendus*.

Themistius. The Philosopher you have nam'd has indeed another Weapon to destroy the Errour about Cold, which he confutes. And this Argument like a two-edged Sword that cuts on both sides, does not only confirm what he maintains, but destroy the chief objection that can be made by his Adversaries. The Argument I speak of he proposes in these terms: *Tametsi multa videantur ex sola*

caloris

caloris absentia frigescere, nihilominus nisi frigus exinsecus introducatur, non tam profectò frigescere quàm decalescere sunt censenda. Esto enim lapis, lignum, aut aliquid aliud, quod nec calidum nec frigidum sit, id ubi fuerit admotum igni calefiet sanè; cùm deinceps calor excedet, neque frigidum ullum circumstabit, non erit cur dicas ipsum frigefieri potius quàm minùs calidum fieri, redirève in suum statum.

Carneades. Whether this contain not a dispute *de modo loquendi*, I shall leave the Company to judge by what I shall return in answer to it. I say then, that it seems to me, that there is in the Discourse an Obscurity, if not an Ambiguity, though I am confident not affected by the Candid *Gassendus*. But to answer as directly as I can; If we speak only of a Coldness as to Sense, I see not, why Water or Wood or any such Body that is heated by the Fire, may not upon its removal thence be said to grow Cold and not barely to *decalescere* in our Philosophers sence of that word. For the Heat and Coldness of Water, in reference to Sense, consisting, (as I lately shew'd) in this, that the Particles of it are more or less agitated than the Hand that is immersed in it, they need nothing else to make the Liquor grow Cold, than such an imminution of the brisk motion of its Corpuscles, that they cease to be as much agitated as those of our Organs of Feeling: And if this already impair'd agitation be still more or more lessen'd, the Liquor will still grow colder and colder without the help of any Positive Cause, 'till at length the agile parts, that kept it fluid, being quite expell'd or disabled, the form of the Liquor comes to be exchang'd for that of Ice.

Phi-

Philoponus. But what say you to that part of *Gassendus*'s Argument, where he proposes an Adiaphorous Body, which, when affected with an adventitious Heat, would not grow cold by the bare removal or cessation of that Heat, unless it were refrigerated by an Agent, that were positively and actively Cold?

Eleutherius. I say, *Philoponus*, this Supposition should not be made, and that I know of no such Adiaphorous Body. For since, as I have been oblig'd to inculcate, those Bodies must be Cold as to sense, whose parts are less agitated than those of our Hands, and consequently Metals, Stone, Wood and other Solid Bodies, and also Water, Wine and all other unmingled Liquors we know, being heated by the Fire, will grow cold again of themselves, because the adventitious motion ceasing by degrees, either upon the recess of the Igneous Corpuscles, or the imparting of the extraneous agitation to the Air or other contiguous Bodies, the Stone or Water, *&c.* will again have so much fainter an agitation, than that of a mans Sensory, as to be by him judged Cold: And because almost all the *Species* of permanent Bodies here below that are known, have in what is call'd their Natural state a less degree of Agitation of their Insensible Parts than mens Organs of Feeling are wont to have those Bodies may be said to be Naturally Cold, and therefore ought not to be suppos'd to be indifferent to Cold or Heat.

Philoponus. But whether or no Nature do really afford us an Adiaphorous Body; yet surely the mind is able to conceive one, and therefore *Gassendus* may be allowed to suppose such Bodies, and

Car-

arneades may be oblig'd to answer what he argues pon that Supposition.

Carneades. 'Tis one thing to propose an Adiaorous Body, as barely an intelligible or a possible ing; and another, to give Instances of it, as *Gasndus* has done in particular Bodies, in which that difference is not to be found. And 'tis this last nd of Supposition that I disallowed in *Gassendus*'s rgument. But if a Body should be proposed as Adiphorous in reference to Heat or Cold, I might say ithout prejudice to my Cause, that if such a Body ould be carried into a hot place, it might there ow warm; and if it should be removed back a-in, and kept till it lost that new adventitious eat, it might rather *decalescere* than grow cold as Sense. But the reason is, because 'tis not every egree of imminution of Heat that is able to denoinate a body Cold, but such a degree as reduces e parts of it to a fainter motion than is at that me in those of our Organs of Feeling; and till is be done, or at least very near done, the proosed Body is still (if I may so speak) in the state f Heat as to Sense: Which last words I add, beause that in reference to other Bodies it may then e notably refrigerated. As Lead that has but heat nough to keep it in Fusion, may, by the pouring n of such Water as to a mans hand would feel Hot, e brought to grow hard, which loss of Fluidity is lso the Natural Effect of Cold, though perhaps both he Metal and the Liquor be yet as to Sense consilerably Hot.

Eleutherius. So that, according to you, none of he kinds of Bodies that are actually known in Naure, are Adiaphorous as to Sense. On which occasion

sion let me note by the by, that the frequent Va-
riations of Sense must render it but an uncertai
standard of Heat and Cold: And upon suppo
sition, that there were an Adiaphorous Body in re
ference to our sense; yet it would not be so in re
ference to all other Bodies, or, in the phrase of o
Verulam speaking of Heat, *in ordine ad Univer-
sum*. And for what remains, the controvers
grounded on *Gassendus*'s Argument seems to be ra
ther Verbal than Real, and may be determined
composed by setling the distinct acceptions of th
words *Cold* and *Heat*.

SECT. VII.

Philop. WHerefore I wish, that we may n
waste the little time that is left u
upon Niceties of no greater concernment; and
think this short time would be better imployed,
Carneades would be pleased to tell us a little mo
particularly, what he supposes to be the thing th
with-held Mr. *Boyle* from delivering an opinio
about the *Nature* of *Cold*.

Eleutherius. Yet me thinks 'tis but fair, th
Carneades, who has all this while been confin'd t
the answering anothers Arguments, should no
take his turn to propose his own.

Carneades. I find in each of your motions, Gen
tlemen, something so equitable and so expedien
that I shall in part comply with both. And that

ma

may haſten to do what *Philoponus* deſires, I ſhall do no more than briefly point at two things that may be alledged in favour of the Hypotheſis I defend. For if you reflect upon what we have already diſcourſed, we may take notice of things there, that will ſcarce be well accounted for by being aſcribed to Poſitive Cold, but may be far better explained agreeably to our *Hypotheſis*. And I muſt add in the next place, that I, who ſuſtained the perſon of a Reſpondent, may pretend to have ſufficiently diſcharged my Office, if I have ſhewn the invalidity of all the Opponents Arguments; and 'tis his part who aſſerts a poſitive thing in Nature, to make it good, whereas he that denies it, needs not alledge any other reaſon why he does ſo, than the Authority of that juſtly received Axiom in Philoſophizing, *Entia non ſunt multiplicanda abſque Neceſſitate*. And, I hope, there will need no other Engine to demoliſh an ill-formed and proofleſs Opinion about Cold, than an *Axiom* ſo Solid and Efficacious, that in the Opinion of almoſt all the Modern Naturaliſts it has been able to aboliſh ſuch potent and immenſe Bodies as the *Primum Mobile* it ſelf, and a ſuperior Orb or two, the leaſt of which contained that Firmament, in compariſon whereof the whole Earth is but a point. And not only ſo, but the ſame Axiom has baniſhed the Angels and Intelligences from the Celeſtial Orbs, that *Ariſtotle* and his followers had aſſigned them to turn about; or rather hath releaſed thoſe Noble and Happy Spirits from the drudgery to which the Philoſophers of ſo many ages had needleſly doom'd them.

Eleh-

Eleutherius. I the less distrust the validity of the Axiom you alledge, because I observe it to be the ground, on which is built a great part of the Reformation of Philosophy, that is introduc'd by the Moderns. For one of the main things that first moved considering men to seek for more satisfactory Opinions than those of the *Peripatetick* Schools, was, that these obtruded a great many Tenents in Philosophy, that were not only un-proved, but unnecessary to the Explication of the *Phænomena* of Nature; as 'twere not difficult to shew.

But I see *Philoponus* preparing to renew the motion he lately made, in which the shortness of time makes me now think it seasonable to joyn with him, I being no less desirous than he to know, what may be the motives of your Friend's declining to declare himself fully about the Nature and Cause of Cold.

Carneades. I have already intimated to you at the beginning of our Conference, that he is himself the fittest person to be addrest to for satisfying this inquiry. But not to be altogether silent on this occasion, I shall tell you, that, as far as I can guess, he waits till farther Tryals and Speculations have resolved him in some points, wherein he is not yet satisfied: For, being of a temper backward enough to acquiesce without sufficient Evidence, when the inquiry is difficult and the subject important; he seems to me to be kept in suspence, both by some Speculative doubts, and the *Phænomena* of divers Experiments, some of which are not deliver'd in his Book. It would be now improper to mention the

scruples

cruples and hesitancies they have occasioned in him; though of those, I have heard him speak of, I shall name some Instances that occurr the most readily. As I remember I heard him make inquiry, as to those that would have Cold produced by Corpuscles of Cold; *Whether*, and on what account, those little fragments of Matter are cold? *Whether* those frigorifick Particles, that must in multitudes crowd into Water to turn it into Ice, have Gravity or Levity, or are indifferent to both? And how any of the three Answers that may be made to this Inquiry, will agree to some *Phænomena* that may be produced? *What* Structure the Corpuscles of Cold can be of, that should make them frigorifick to that innumerable variety of Bodies they are said to pervade? And *whether* the frigorifick faculty of these Corpuscles be loosable or not? As also *whether or no* they be Primitive Bodies, and if it be said, they are not, whether there was not Cold in the World before they were produced, and whence that Cold could proceed? And if it were said they *are* Primitive Bodies, he demanded, *how* it came to pass, that, by putting a certain factitious Body actually warm into Water that was also warm, (both which appeared by a good sealed Weather-glass,) there should presently be produced an actual Coldness (discernable by the same Thermoscope?) These, and I know not what other Scruples and Difficulties, suggested to him by his thoughts or his Experiments, were the things that I suppose prevail'd with a man of his temper to forbear for a while the declaring of his Sentiments about Cold; lest the Event of some

ſa ther tryal ſhould ſhew him cauſe to retract them.

Philoponus. What you have freſhly intimated, *Carneades*, of Mr. *Boyle*'s having other heſitations than thoſe you have named and ſuggeſted by Experiments not publiſhed in his Hiſtory, does, I confeſs, the more excite my Curioſity to have at leaſt a taſte of thoſe perplexing *Phænomena.*

Carneades. You may eaſily gueſs, *Philoponus* by what I have told you already, that you are no to expect a full ſatisfaction from me on this occa ſion. But yet, that your curioſity may not b fruſtrated, I ſhall venture to acquaint you wit two *Phænomena*, which were, I ſuppoſe, none o the leaſt motives of his backwardneſs to declar himſelf. But though ſome body perhaps think that the grounds of ſolving theſe *Phænomena* an moſt of the newly recited Scruples, may be pick out of ſome things that may already have paſs among us in this Conference; yet becauſe we hav not now time to enter upon a diſcuſſion of this mat ter, I am willing you ſhould ſuſpend the debate till we have occaſion to meet another time; an therefore I ſhall now only acquaint you with a cou ple of Experiments, that he ſet down for a *Virtuſo* who was to ſolve the two main Problems ſuggeſte by them. The *firſt* whereof was, Whence Wate ſhould upon Congelation acquire ſo vaſt a force a he found it had to lift up great Weights and bur containing Bodies; though it ſeem'd by ſeveral Circumſtances, that the motion of the Water very much diminiſhed when 'tis changed into Ice And the *ſecond* Problem is thus conceived; If, a briſk agitation of a Bodies inſenſible parts pro

duce

duces Heat, so the Privation of that Motion is, as *Cardan* and the *Cartesians* would have it, the cause of Cold; whence is it, that, if certain Bodies be put together, there will be a manifest and furious agitation of the small parts, and yet upon this conflict the mixture will not grow hot, but sensibly and even considerably Cold? The Narratives themselves of the Experiments are too long to be now read over to you. And therefore I shall leave the Paper, that contains them, among you, to be perused at your leisure, between this and our next meeting, till when I must bid you farewell: Only desiring you in the mean while to remember, that, as I have but acted a part imposed upon me in our past Conference, so notwithstanding any thing that I have said in my assum'd Capacity, I reserve to my self the right of appearing as little preingaged as any of you at our next meeting.

FINIS.

TWO PROBLEMS ABOUT COLD,

Grounded on

NEW EXPERIMENTS,

And Proposed

In a LETTER to a FRIEND.

By the Honourable *ROBERT BOYLE*.

TWO PROBLEMS

ABOUT

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NEW EXPERIMENTS,

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In a LETTER to a FRIEND.

By the Honourable ROBERT BOYLE.

To my very Learned Friend Mr. J. B.

SIR,

I Presume that you will not be surpriz'd to be told, that I send you the inclos'd Papers, not only that I might gratifie your Curiosity, but that you may by them be inabled to help me to satisfie my own; and therefore I shall accompany the Historical Transcripts I made of the following Experiments, as I found them registred for my own Remembrance, with some of the doubts suggested to me by some of the *Phænomena* that occurr'd. But yet I shall not trouble you with all the difficulties that at first troubled me, but reduce the Exercise, I desire to give your sagacity, to the solution of two Problems. And I will begin with propounding that first, which is grounded upon the last of the two following Papers, because though the Historical part of that be much the longest, yet the grounds of my *Quære* concerning it, will be much more briefly propos'd, the Experiment it self naturally suggesting this Problem; *How upon the mixture of two or three Bodies,* such as those mentioned *Problem.* II. in the Paper, *there should manifestly*

enſue a great and tumultuary agitation of ſmall parts, and yet even during this conflict, not any ſenſible Heat, but *a conſiderable degree of Cold be produc'd*, and that even in the internal parts of the mixture?

The Inducements to make this Problem need not be far fetch'd, it being obvious enough, that according to the Corpuſcularian Philoſophy, which you and I agree in, a brisk and various agitation of the minute parts of a Body is that, which makes it Hot both in reference to our Senſories, and to its operations on other Bodies. But I doubt, the riſe of the Problem is much more eaſie to be underſtood, than the Cauſe of the *Phænomenon*, about which I will not ask you, Whether one may not aſſert, that Local motion is in its own nature a Generical thing, which may be ſo diverſified by Circumſtances, that one kind of Modification of it, as 'tis made in Corpuſcles of ſeveral ſizes and ſhapes may be the cauſe of Heat, and another that of Cold? Or elſe, Whether we may ſuppoſe, that Cold is a poſitive thing, and operates by real Corpuſcles of Cold, which happening to abound, and yet to be lock'd up in the Bodies whoſe mixture I imploy'd, they are, by the great conflict that diſſolves the Texture of the Claſhing Salts, ſeparately put into motion and that in ſuch numbers, that though really there would be a Heat produc'd by the brisk and confus'd agitation of ſome of the parts, yet that Heat is not only conceal'd and check'd, but maſter'd by the over-powering operation of the Frigorifick Corpuſcles. But to ask you about this or any other particular way of ſolving our *Phænomenon*, were to forget, that my aim is to

learn

learn not your opinion of this or that particular Conjecture or fancy about our Problem, but in general, how it may be beſt reſolv'd, and what you think to be the true Cauſe of ſo odd an Effect.

Having thus diſpatch'd the little I had to ſay about the Paper that ſuggeſted the ſecond Problem, I will now ſuppoſe that you have read the *Phænomena* that contain the riſe of the firſt, to which I ſhall proceed without farther Preamble, ſince the Queſtion or Problem, that theſe naturally call for, is, *Whence this vaſt force of freezing Water proceeds?* *Problem* I.

For, the breaking of reſiſting Bodies being to be made by a violent Local motion, and Cold, according to the Judgment even of the Moderns, either conſiſting in, or at leaſt being accompanied with, a Privation, or a great Imminution of Motion, it ſeems very difficult to conceive how Cold ſhould make Water to exert ſo wonderful a force. I know the Learned *Gaſſendus* and divers other Philoſophers teach us, that Glaciation is perform'd by the entring of ſwarms of Corpuſcles of Cold, as they call them, into the Liquor. But I much doubt, Whether from this Hypotheſis a good Solution of our *Phænomenon* will be deriv'd, ſince theſe Atoms of Cold ſeem not barely as ſuch to make that Expanſion of the Water, which is requir'd in the Experiment by me recited. For I ſee that though Water will be more and more refrigerated, according as the Air grows colder and colder, yet till it be brought to an actual Glaciation, all the ſwarms of the Frigorifick Atoms in it, are ſo far from expanding it, that they more and more condenſe it. And even that degree of Cold which deſtroys Fluidity, though it expands

Water,

Water, does not do it merely by the multitudes of the Frigorifick Corpuscles that invade the Pores of the lately fluid Body, since pure Spirit of Wine and almost all Chymical Oyles, though expos'd to the same degree of Cold that turns Water into Ice, or as I have tryed, unto a far greater than is necessary to do so, will be but the more condens'd by those swarms of Particles. But, which is more considerable, I have carefully observ'd, that, besides common or expresst Oyls, Chymical Oyl of Aniseeds it self, being frozen or concreted by an intense degree of Cold, will not be expanded but notably condens'd, and accordingly grow specifically heavier than before. And this was one thing that kept me from expecting the removal of our Difficulty from the Ingenious Explication given of Freezing by the *Cartesians*, when they teach, that the Eel-like particles whereof they suppose Water to consist, are very remissly agitated, and their want of pliantness makes their Contexture less close; which yet seems not to agree with the lately mention'd Tryals. And though these Eel-like particles should lose all their flexibleness, though in that case it may probably be said, that they would take up less room than before, if nothing oppose their Expansion, yet it does not thence appear, how they should acquire so vast a power to expand themselves in spite of Opposition, as we have shewn Water by Freezing does acquire.

I did not hope to resolve our Problem by the help of a Vulgar Supposition, that well-stopp'd Vessels are broken in frosty weather *ob fugam Vacui*, since I found that Supposition to be erroneous by divers Experiments, some of which are mention'd in the History of Cold.

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It seem'd less improbable, that some assistance to the solving of our difficulty might be given by two other things. Whereof the first is, That, for ought I have yet observ'd, no Liquor but Water, or that which participates of Water by having Aqueous Particles separable from it, will be made to swell by Cold; nor will Water it self do so upon every degree of Cold, but only upon so great an one as actually turns it into Ice. And the second is, That upon the Glaciation of Water and Aqueous Liquors, we may observe in the Ice many bubbles greater or smaller intercepted between the Solid parts, and suppos'd to be full of Air, (I say suppos'd, because upon tryal I found them to have yielded but a small proportion of common Air;) which supposition, if true, would perhaps invite one to suspect, that the Air contain'd in these bubbles might have an interest in our *Phænomenon*; since I have found by tryals purposely made, that Air congregated into Visible though not great portions, may exercise a considerable Elasticity, which appear'd not whilst 'twas insensibly dispersed through the Water.

And if I did not suppose, both *that* you had taken notice, that there are wont to be numerous particles of springy Air dispers'd through the Pores of Water; and *that* you had consider'd, *whether* the want of pliantness occasion'd by Cold in the Aqueous Corpuscles, whilst they are yet agitated and brandish'd by some permeating matter; and *whether* upon the change of the Pores, that we may conceive to be made in freezing Water, either by the recess of one sort of subtil Corpuscles or the admission of another, or the closer constipation of the grosser parts, there may not be produc'd in Corpuscles, that compose

Wa-

Water, (to say nothing of the intermix'd Air, or th Concretions or the Coalitions occasion'd by th Cold,) a springiness capable to make many littl Bodies, endow'd with it, exert a great force against the sides of the Vessel, that oppose their joynt en deavour to expand themselves: If, I say, I did no believe, that these and the like suspicions had oc curr'd to you as well as to me, together with th difficulties wherewith each of them seems to be in cumber'd, I would acquaint you with what thought and tryals occurr'd to me about these and the lik conceits. But I not daring to think this coul prove other than a needless work, I must remember that my business in this Paper is to propose Diffi culties, not the wayes of solving them; it bein from your Kindness and Sagacity, that these are a well expected as desir'd by,

SIR,

Your, *&c.*

AN ATTEMPT

To Manifeſt and Meaſure the

GREAT EXPANSIVE FORCE

OF

Freezing Water.

By the Honourable *ROBERT BOYLE.*

AN ATTEMPT

To Make and Measure the

REAL EXPANSIVE FORCE

OF

Freezing Water.

By the Honourable ROBERT BOYLE.

AN ATTEMPT

To Manifeſt and Meaſure the

GREAT EXPANSIVE FORCE

OF

Freezing Water.

COnſidering when I writ the Hiſtory of Cold, that though divers *Phænomena* might induce an attentive Obſerver to think, that Freezing Water had an Expanſive Force, yet I had not met with any that endeavour'd, or even ropos'd, to meaſure it, whether becauſe they re-ected not on it at all, or judg'd not the Force onſiderable; I who look'd with other eyes upon , thought fit to repair that omiſſion, but was then ill furniſhed with requiſites for doing it fully, hat I remember I complain'd of it in my Hiſtory of Cold. And though, even afterwards, when the ime of the year was favourable, I could not pro-ure ſuch Accommodations as my deſign exacted; et thinking an imperfect way of Meaſuring to be etter than none, I preferr'd to the making no at-

tempt

tempt at all the endeavouring to do what the least defective Instruments, I could procure, would permit me, towards the making an estimate by known Measures, of the Expansive Power of Freezing Water. For though I did not expect, I should be able accurately to define it; yet I hop'd I should make such an estimate as to know that Force not to be, as one would think it, Faint and Contemptible, but very Great and Considerable.

I remember on this occasion, that to manifest the Force of Freezing Water, I caused the Barrel of a short Gun to have a skrew fitted to the Nose of it, by which we might exactly stop it, as we did the Touch-hole another way; then filling the Barrel with common Water, and closing it accurately by the help of the skrew, we laid it in a conveniently shap'd Vessel, wherein we incompass'd it with a Frigorifick Mixture (of Snow or Ice and Salt,) and in a short time we found, as we expected, the Barrel to be burst, part of the Ice appearing along the gaping slit that had been made in the Body of the Iron by the Freezing Water, which by this Effect seem'd to emulate the justly admir'd force of kindled Gun-powder. But the Design of this short Paper tending not so much to *prove*, as (in some sort) to *measure* the Expansive Force of Water, I shall subjoyn the Transcripts of two or three Experiments, made chiefly for that purpose.

EXPERIMENT I.

[There was taken a strong Cylinder of Brass, whose Cavity was two inches in *Diameter*, into this was put a Bladder of a convenient size, with a quanti-

quantity of Water in it, that the neck of the Bladder (which I had taken care to have oyl'd) being strongly tyed, the Water might not get out into the cavity of the Cylinder, nor be capable of expanding it self some other way than upwards. Then into this Cylinder was fitted a Plugg of Wood, turn'd on purpose, which was somewhat less in Diameter than the Cylindrical cavity, that it might rise and fall easily in it. Upon the upper part of this Plugg was laid a conveniently shap'd flat Body, upon which were plac'd divers weights to depress the Plugg, and hinder its being lifted up by the Expansion wont to be made in Water that is made to freeze; then a frigorifick Mixture being afterwards apply'd to the Cylinder, it appear'd within half an hour or somewhat more, by a Circle that had been purposely drac'd on the side of the Plugg, where 'twas almost contiguous to the Orifice of the Cylinder, that the Water in the Bladder began to expand it self, and about two hours after, having occasion to shew the Experiment to some inquisitive persons, the circle appear'd to have been heav'd up in my estimate about $\frac{3}{8}$, if not half, of an inch, notwithstanding all the weights that endeavour'd to hinder the ascension, though these weights amounted to 115 pound, which were all the determinate weights we could then procure, besides a brick and some other things that were estimated at five pound more; nor did I doubt that a far greater Load would not have hindred its Expansion.]

EXPERIMENT II.

[We took a Brass Cylinder, whose Dimensions were three inches $\frac{8}{10}$ in Diameter, and in depth four inches. Into this we put a fine bladder of a convenient size, almost fill'd with Water, and strongly tyed about the neck; upon this bladder we put the wooden plugg to stop up the Orifice as much as was convenient, and upon the plugg we put a piece of a flat board for the weights to stand upon. These things being prepar'd, we convey'd the Cylinder with all that belong'd to it save the board, into a large wooden Bowl, where we applied to the Cylinder a good quantity of the Frigorifick Mixture, made with beaten Ice and Bay Salt; and having first mark'd with a circular line the Edge or Contact, where the orifice or lip of the Cylinder touch'd the Plugg, we layed on the weights upon the board, and when by their weight they had depress'd the Plugg till the cover of it lean'd upon the Cylinder, we dispos'd our selves to attend the issue of the Tryal. The event whereof was this, that when the action of the Frigorifick Mixture had produc'd some Ice in the Water included in the Bladder, that Liquor appear'd to have dilated it self strongly enough to begin to raise the Plugg with the superincumbent weights, and by degrees they were by the growing Ice rais'd till the mark, diligently made on the Plugg where the edge of the Cylinder touch'd it, was about a tenth part of an inch above the station it had before the Plugg had been depress'd. Then we took out the Bladder, and found the Cylinder of Water within the Bladder not t

be wholly turn'd into Ice, but to contain some quantity of unfrozen Water in the parts about the Centre, which Liquor, if we had not so soon desisted from the Experiment, (as for certain Reasons we did) might probably have rais'd the weights somewhat higher. But as it was, the Ice in length was but three inches and about $\frac{1}{8}$, and yet so small a quantity of Ice suffic'd to raise, besides the board they lean'd on, as many weights of Lead as amounted to an hundred pound *Averdupois*.]

EXPERIMENT III.

[The day after the above mention'd Experiment was made, to try yet farther the Expansive force of Freezing Water, the same was reiterated after the manner above deliver'd, but with this difference, that, having procur'd more weight, when the Plugg was lifted up $\frac{1}{8}$ or somewhat better (which Plugg began sensibly to rise within half or three quarters of an hour after the Frigorifick Mixture was applyed,) it was loaded with a weight of two hundred pounds, and a fifteen pound piece of Lead, and other Bodies, as Boards, *&c.* to lay the weights upon, which being also weigh'd by themselves came to fifteen pound more, so that the whole amounted to 230 pound; and if the hundred pounds were both of them, as their bulk and shape invited us to guess, of that sort of weights which are call'd the Greater Hundred, containing an hundred and twelve pound a piece, twenty four pound must be added to the samm, which would thereby be made up 254 pound.]

A NEW EXPERIMENT ABOUT THE PRODUCTION of COLD BY THE CONFLICT of BODIES, Appearing to make an EBULLITION.

By the Honourable *ROBERT BOYLE.*

An Advertisement of the Publisher's.

'Tis manifest enough by the beginning of the following Paper, that 'twas not intended to come abroad alone, as indeed it was but a part of some Writings about Cold, *design'd to inlarge the History of that Quality. But yet the Author forbore, by altering it, to accommodate it to the Papers wherewith it now comes forth; because in this very form it was by him (being to take a journey) left seal'd up with the Learned Secretary of the* Royal Society *in* Febr. *in the year* 166$\frac{4}{5}$; *since when it did, till lately, continue in those safe hands; the Author having no need to make use of it. Which Circumstances are now mention'd to keep the* Reader *from wondring, that the Author speaks of the Production he made of Cold by the Conflict of two Liquors, as a New Experiment and* Phænomenon; *though now two or three years ago, the learned* Sylvius, *as he is inform'd, takes notice in one of his Books, of a way of producing Cold by a Mixture of Spirit of Vitriol, and another Saline Spirit. But besides that the Author's way is differing enough from* Sylvius *his, 'tis apparent by the time when his Experiment was left with Mr.* Oldenburg, *who is ready to bear witness to what is here said, that he had made it at least some years before the other, to which he was perfectly a stranger nor hath yet ever tryed it, came abroad. Nor should he easily have look'd for the Production of Cold by the mixture of the Acid Spirit of Vitriol with every Volatile Spirit; because he found that the Oyl of Vitriol (as that Acid Liquor is commonly, but abusively, called) would by its conflict with Urinous Spirits, produce not Cold but Heat. Whether the Care and Cautiousness, with which he made the Experiment now to be subjoyned, may give the Diffident and Curious more satisfaction than a bare Affirmation would do of his having produced Cold upon a mixture of contrary Bodies, he leaves it to others to judge. And I shall now only add, that he some years since shew'd some Curious Persons, that* Cold *may be made to result from the Conflict of Bodies, whereof none is a necessary Ingredient in the Experiment, from which, it may be, I have too long detain'd the Reader.*

A NEW EXPERIMENT ABOUT THE PRODUCTION of COLD BY THE Conflict of Bodies, appearing to make an Ebullition.

AND now that we are searching after the Nature of Cold, I am put in mind that I have sometimes wondred at a certain Experiment that is so Anomalous, and seems so little of kin to the usual *Phænomena* of Cold, that though I do not particularly teach the way of making it, because I could not do it without discovering something in Chymistry, that cogent considerations forbid me at present to publish; yet I cannot forbear to relate, on this occasion, the matter of Fact, *both* because it may afford considerable Hints to sagacious Inquirers, *and* because it seems so little congruous to most Theories of the *Causes* of Cold, that it may make the Framers of Theories

more wary, and help also to excuse my backwardness to propose *Hypotheses* about Cold in a resolute and confident way.

The Experiment is this: We took three Saline Bodies, each of them purify'd by the Fire; and whereas there are divers Bodies, that being mingled together acquire a Heat, which neither of them had apart; and whereas it is said by some that there are a few, which being blended together make a mixture somewhat colder than either of themselves, these Salts of ours being put together in due proportion, do upon their mixture produce that, which the Eye judges to be a great Effervescence; but though the hissing noise be loud, and though the numerous Bubbles suddenly generated will make the matter apt to overflow the Glass, if the one be not capacious, and the other be not put in by little and little; yet even whilst this seeming Ebullition lasts, the Glass, which one would expect to find very hot, (as usually happens upon the mixture of the Silt of Tartar, and Spirit of Nitre, and upon the confusion of the like Saline Bodies dispos'd to produce together such Efflorescencies) instead of growing hot, does, if it be held in ones hand, feel much cooler than before, and that in a wonderful degree; insomuch that ev'n in Winter the outside of the Glass would quickly be cover'd with great drops of Dew, which after a while would unite, and trickle down by their own weight. And this we could make to last for a great while, by casting in by degrees more and more of one of the Ingredients on the other. And besides that, this copious Dew on the outside of the Glass, reach'd as high as the mixture within, which argued whence it proceeded;

be-

besides that, purposely looking on the bottom of the Glass whose outside was concave, we found no such drops of Dew there, because the Vapours of the External Air could not in any quantity have access to it; which shew'd the Dew, conspicuous elsewhere, not to come from the transudation of the finer parts of the Mixture through the pores of the Glass: Besides these things, I say, I remember, that having sometimes purposely wip'd off the Dew here and there with my Handkerchief, the dry parts of the Glass would in no long time regain fresh drops of Dew. And this odd Experiment we did for the main repeat not only in the presence of an Industrious Chymist, (whose Tryals unexpectedly gave us the Rise of the Experiment,) but also alone, and at differing seasons of the year.

I shall add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the Experiment, because I could then make use of a seal'd Weather-glass, which I had not at hand when I made the former Tryals; I took two deep Glasses, into the one of which I put a good quantity of fair Water, and in the other I made such a Mixture as I was lately mentioning; and having by a string, (to prevent the altering of the temper of the included Air by the warmth of my fingers) let down the Weather-glass into the Water, that the Liquor shut up in the Instrument might be cool'd by the ambient Water; after it had stay'd there a reasonable time, I took it out by the string that was fastened to the upper part of it, and letting it down into the mixture that was then hissing, and filling the Vessel that contain'd it with multitudes of successively emerging and hastily vanishing

niſhing bubbles; I perceiv'd nevertheleſs, that th coldneſs of the ſeemingly efferveſcent mixture mad the impriſon'd tincted Liquor to ſubſide ſo low that from four inches and three quarters (or there about) at which height it ſtood in the carefully di vided ſtemm, when the Weather-glaſs was take out of the Water, it fell in a ſhort time lower tha to one inch and a half. And becauſe I foreſa that this might ſeem ſcarce credible, eſpecially if ſhould relate how ſwiftly the impriſon'd Liquo ſubſided at the beginning; I ſhall annex, that, fo farther ſatisfaction of others, I remov'd the Ther mometer out of the mixture into the common Wa ter again, where it ſoon reach'd to ſomewhat abov four inches and a half; and not content with tha I put it a ſecond time into ſome of the frigefactiv mixture before it had done foming, in which it fell as before, ſomewhat below an inch and a half, and preſently after, almoſt as low as to an inch. An having once more put it back into the Glaſs tha contain'd the Water, the included Liquor re-aſ cended to above four inches and a half, and thi in an excellent ſeal'd Weather-glaſs, whoſe ſtemm was not in all above ten inches long, with a Bal proportionably big. And for farther confirmation I took notice, that, whilſt the mixture, by its hiſſing noiſe, and its ſtrangely numerous Bubbles, ſeem'd to be in a ſtate of Ebullition, the outſides of th Glaſs that contain'd it, were, as far as the mixture reach'd, ſo plentifully bedew'd with the condens'd Vapours of the ambient Air, that their weigh carried them down in little ſtreams which lef round about the bottom of the Veſſel a pretty quan tity of Liquor, that appear'd by its taſte not to have been

een made by the transudation of any of the sharp
nd Saline Liquors that were agitated within the
lass. There remain'd only one scruple, which was
ggested to me by the remembring of a circum-
ance, which however, at the making of the fore-
tention'd Tryals, I had not minded, and which
ossibly most Observers would have neglected;
ut calling to mind, that the Water, I had made use
f to immerse the Weather-glass in, was brought
at of a room wherein a Fire was wont (though
ot constantly) to be kept, whereas the Ingredi-
nts of the mixture were kept, and put together in
Chamber, which, though contiguous to the for-
er, had no Chimney in it; I thought fit, for great-
r circumspection sake, to let the Water stand all
ight in this last-mention'd Chamber, that the Am-
ient Air might have the same Operation upon it,
s upon those Bodies that were to be Ingredients of
he mixture: And then repeating the formerly re-
ited Experiment, though I thought it needless to
pend time to watch, as before I had done, the
reatest difference in Cold betwixt the Water and
he bubbling Mixture; yet by making removes of
he Weather-glass to and fro, from one Liquor to
nother, it sufficiently appear'd, that the greater
oldness, remarkable in the mixture, did not be-
ore proceed in any considerable degree (if in any
legree at all) from the Water's not having been
ept in the same Room with it.

So that by these different Tryals it seems mani-
fest, That the coldness of the mixture was not a
Deception of the Sensory, since it would be disco-
ver'd by the operation, it had, not only upon the
Vapours of the Air on the outside of the Glass, but
upon

upon the Thermometer it self, plac'd in the midst of the mixture, which this last nam'd circumstance argues to have been cold throughout, and ev'n in its innermost parts.

And to shew, how much this strange coldness depended upon the peculiar Texture of the mixture or the structure of its component Corpuscles, and the peculiar kind of motion that was excited in the tumultuating Particles; I shall here subjoyn a Relation which probably will not appear despicable; namely, That in the first place I took some of the acid Liquor, the rest of which I had made use of to make the mixture, whereof I have been speaking; and put a convenient quantity of fair Water, which had been kept a night or two in the same room (wherein was no Chimney) with it, that there might be no cause of suspicion, that the one had been expos'd to a more or less cold Air than the other; and yet these two Liquors did scarce sensibly differ in coldness; though to discover whether they did or no, I remov'd from one to another of them a good seal'd Weather-glass with a very slender stemm.

And in the next place, I took a convenient quantity of the pure Salt, I had so often employ'd, and cast it into a Glass full of Water, which I had kept many hours in the same room with it, and wherein I had a little before plac'd a seal'd Weather-glass, that the included Liquor might be brought to the temper of the Ambient Liquor; but upon this Injection, the tinged Liquor of the Thermoscope subsided so little, as not to make me look upon this Salt as being it self extraordinarily Cold, since other obvious Salts (that I have at other times

cast

aſt into Water to cool it a little) and ev'n Sea-alt would (according to my Eſtimate) have re-rigerated it as much, if not more. Nor did I ob-erve the Glaſs, wherein I was wont to keep ſtore of our Salt, (though I had often occaſion to handle t) diſcloſe to the touch any remarkable degree of Coldneſs; ſo that the coldneſs of our hiſſing mix-ure could not be attributed to that of either of the ngredients apart, but was a Quality emerging up-n their being blended. Now when I thus made heſe Preparatory Tryals, having afterwards plac'd n the ſame Window (of the Chamber laſt menti-on'd) a couple of Glaſſes, with common Water in ne, and in the other ſome of that mixture, of vhoſe frigefactive power I had very recently made Tryal; I left them to ſtand there together all night, nd left alſo ſtanding by them ſuch a ſeal'd Wea-her-glaſs as I have been mentioning; and the next norning, when all the viſible commotion or agita-ion of the minute parts of the contrary Salts of he Mixture was quieted, I put the Weather-glaſs irſt into one of thoſe two Liquors, and then into the other, and after remov'd it back into the for-mer again, without perceiving any difference worth minding betwixt the coldneſs of the mixture and that of common Water: And with much the like ſucceſs I repeated the Tryal, after the Water and the other Liquor had ſtood in the ſame room (unfurniſh'd with a Chimney) for near two dayes and nights.

And for farther confirmation, I ſhall add, that having inſtead of the Salt, which I hitherto made uſe of, taken ſome of the Spirit, that was wont to come over together with that Salt, and did ſo abound

with

with it, that a good deal of it lay undissolved at th bottom of the Liquor; having, I say, imploy'd thi saline Spirit instead of the Salt it self, and havin for Tryals sake mix'd with it another Spirit, draw in my own Laboratory for the purpose, which t me seem'd as like, as could be made, to that whic I had all this while made use of; I found, that th mixture of these two Liquors (though it produc far fewer Bubbles than I was wont to have) instea of growing Cold, grew Luke-warm, and quickl impell'd the Liquor in the Weather-glass, from little above three inches, to as much above eight and yet, besides that this last Spirit was, as far a I could perceive, and that after the same manne drawn from the same Materials with that I had us all this while; the *Smell* and *Taste*, (which ar both of them peculiar and odd enough) concurr to manifest the two Spirits to be of the sam kind.

And, for farther proof, I shall add, that to satisfi my self the more fully, I took a parcel of the sam Liquor, I had lately employ'd with success in ma king the Frigorifick Mixture, and yet ev'n this Li quor, which with the dry Salt would questionles have produc'd a Frigefactive Mixture as well as th rest had done, which I had a little before taken ou of the same Viol; this Liquor (I say) put to a new portion of the Saline Spirit above-mention'd. though they did not produce minute Bubbles numerous enough to make a Fome; yet the Mixture, instead of growing very cold, grew manifestly Luke-warm, not only in the Judgment of the Touch, but by its Operation on a good seal'd Weather-glass, carefully and for a competent while imploy'd to examine

mine the Temper of it. Whereas on the contrary, having purposely kept some of the Frigorifick Spirit by the Fire side, till its temper was so alter'd, that it nimbly enough rarified and impell'd up the Spirit of Wine contain'd in a seal'd Weather-glass, immers'd in it, and having into this Liquor cast some of the Frigorifick Salt, ev'n whilst the Spirit of Wine was rising, and would probably have risen a pretty while longer; this injected Salt, when it began to be dissolv'd, did not only give a check to the rising Liquor, and quickly put a stop to its ascent; but, (as I expected) soon made it subside again, till it fell about three inches or more (which was very much in a short Weather-glass) beneath the Station where the Spirit of Wine had rested, before the Liquor was set by the Fire side; nay, afterwards, I try'd, That a Frigorifick Salt, being well warm'd by the Fire side, did, with an appropriated Liquor, that was also warm'd, produce a coldness manifestly perceivable by the Weather-glass. So that in these cases a Body but moderately cold, nay actually warm, hastily reduc'd one, actually warm, or at least tepid, to a far greater degree of actual coldness than it self had.

These are some of the Experiments I try'd with the Liquors and Salts, of which, upon allowable Considerations, I must now forbear to set down the way of preparing: But that ev'n at present I may not be altogether wanting to the Curious, I devis'd a way of making a *Succedaneum* to this Experiment, which I shall here willingly annex, as that, which though it be much inferiour to what I may the day be at liberty to acquaint the Reader with; yet it will shew the main thing intended, by manifesting,

festing, That Cold may by the mingling of Bodies be produc'd, or increas'd to a degree exceeding that of either of the Bodies that compos'd the Mixture; and this, though at the same time a seeming Effervescence be made by the Bodies, that thus refrigerate each other.

I took then very good Salt of Tartar, and putting to it a convenient quantity of Spirit of Vinegar, I did, whilst the mixture was hissing, (but seem'd to the touch to have refrigerated the Glass that contain'd it,) immerse into it the Ball of a good seal'd Thermoscope, furnish'd with Spirit of Wine. And though the Weather-glass were not much above a foot long, yet the coldness of this Mixture made the Tincted Liquor descend, hastily enough, two inches and almost a half. And to shew farther That this Mixture was actually colder than cold Water, removing the Weather-glass out of the Mixture into that Liquor, the tincted Spirit began to re-ascend, and that so nimbly, that in about three minutes (that the Ball of the Thermoscope stay'd under water) the Spirit of Wine had re-ascended about an inch and a half, if not more. And to try whether this coldness of the mixture did proceed from, or depend upon, some Texture of the parts, that was not very permanent, and yet did not quite degenerate, immediately after the Ingredients had ceas'd to work upon one another, I remember, that near an hour after the Ebullition of the Spirit and Salt of Tartar was over, the Thermoscope being remov'd out of the common Water, where it had stood immers'd, into the Mixture, descended about half an inch or more. For want of Salt of Tar-

tar

ar I could not begin the Experiment anew, and so im not sure it will alwayes succeed uniformly.* But yet to give my self what farther satisfaction I could, by trying the same Experiment in such a way as might discover, whether or no the *Phænomenon* did not depend upon, or require some peculiar Texture in the fix'd Salt that had been employ'd; I took some Alcaly (made by dissolving Pot-ashes in fair water, and reducing them by coagulation to a white Salt,) and pouring Spirit of Vinegar to it, I found, That this mixture did not, whilst it hiss'd, grow at all colder, but rather somewhat warmer. And, for farther satisfaction, immersing into it the Ball of the newly mention'd Weather-glass, I found, that it ascended in a short time about an Inch, and, being remov'd into the Water, descended about half an inch; and by making removes of it from one of these Liquors into the other two or three times more, I found, That the Spirit of Wine did rise and fall according to what has been newly observ'd, but its motions upwards and downwards were both less than before, and more slow.

** The Author's wariness was not here amiss, he having afterwards found, that this Experiment did not alwayes succeed.*

FINIS.

OBSERVATIONS AND EXPERIMENTS ABOUT THE SALTNESS of the SEA.

By the Honourable *ROBERT BOYLE.*

ADVERTISEMENT

To the following Obſervations (which may alſo ſerve for many Hiſtorical paſſages in the Author's other Writings.)

WHereas the Author does frequently make uſe of the Relations of profeſſed Seamen and other Navigators, and of Obſervations made ſome in the East, *and ſome in the* West-Indies, *it will be fit to advertize the Readers, that he has been very wary in admitting the informations that he imployes; being forward enough to reject, as he has often done, ſuch as many others would gladly have received: But notwithſtanding his wonted rejection of the particulars he ſan cauſe to diſbelieve, 'twas eaſie for him to be well furniſhed with ſuch relations as he makes uſe of; ſcarce any Writer of Philoſophical things having had ſuch opportunities of receiving ſuch Authentick Information from Sea Captains, Pilots, Planters, and other Travellers to remote parts, as were afforded him by the advantage he had to be many years a member of the Council appointed by the King of* Great Britain *to manage the buſineſs of all the* Engliſh Colonies *in the Iſles and Continent of* America, *and of being for two or three years one of that Court of Committees (as they call it) that has the ſuperintending of all the affairs of the juſtly famous* East-Indian Company *of* England.

OBSERVATIONS AND EXPERIMENTS ABOUT THE SALTNESS of the SEA.

THE FIRST SECTION.

Chap. I.

THE Cauſe of the Saltneſs of the Sea appears by *Ariſtotle*'s Writings to have buſied the Curioſity of Naturaliſts before his time; ſince which, his Authority, perhaps much more than his Reaſons, did for divers Ages make the Schools and the generality of Naturaliſts of his Opinion, till towards the end of the laſt Century, and the beginning of ours, ſome Learned Men took the boldneſs to queſtion the common Opinion; ſince when the Controverſie has been kept on foot, and, for ought I know, will be ſo, as long as 'tis argued on both ſides but by Dialectical Arguments, which may be probable on

both sides, but are not convincing on either. Wherefore I shall here briefly deliver some particulars about the Saltness of the Sea, obtained by my own tryals, where I was able; and where I was not, by the best Relations I could procure, especially from Navigators.

First then, Whereas the Peripateticks do, after their Master *Aristotle*, derive the Saltness of the Sea from the Adustion of the Water by the Sun-beams, it has not been found that I know of, that where no Salt or Saline Body has been dissolved in, or extracted by Water expos'd to the Sun or other Heat, there has been any such Saltness produc'd in it, as to justifie the *Aristotelian* Opinion. This may be gather'd, as to the Operation of the Sun, from the many Lakes and Ponds of fresh Water to be met with, even in hot Countryes, where they lye exposed to the Action of the Sun. And as for other Heats, having out of Curiosity distill'd off common Water in large Glass Bodies and Heads till all the Liquor was abstracted, without finding at the Bottom the two or three thousandth part, by my guess, of Salt, among a little white earthy substance that usually remained. And though I had found a less inconsiderable quantity of Salt, which, I doubt not, may be met with in some Waters, I should not have been apt to conclude it to have been generated out of the Water by the Action of the Fire, because I have by several Tryals purposely made, and elsewhere mention'd, found, that in many places, (and I doubt not but if I had farther tryed, I should have found the same in more) common Water, before ever it be exposed to the Heat of the Sun or other Fire, has in it an easily discoverable Saltness

of

of the nature of common Salt, or Sea-Salt; which two I am not here ſollicitous to diſtinguiſh, becauſe of the affinity of their Natures, and that in moſt places the Salt eaten at Tables, is but Sea-Salt freed from its Earthy and other Heterogeneities, the abſence of which makes it more white than Sea-Salt is wont to be with us. *Theſe laſt Words* I add, becauſe credible Navigators have inform'd me, that in ſome Countryes Sea-Salt without any preparation coagulates very white; of which Salt I have had, (from divers parts) and us'd ſome parcels.

But ſome of the Champions of *Ariſtotle*'s Opinion are ſo bold as to alledge Experience for it, vouching the Teſtimony of *Scaliger* to prove, that the Sea taſtes ſalter at the top than at the bottom, where the Water is affirmed to be freſh. But as for the authority of *Scaliger*, though I take him to be an acute Writer, yet, I confeſs that, for reaſons elſewhere given, I do not allow it that Veneration which I find given it by very Learned Men, nor am I over prone, even as to matters of Fact, to acquieſce in what he tells us, when he neither ſignifies that he delivers things upon his own Experience, or declares from what credible Information from others he received them.

'Tis true, that having often obſerved, that Sea-Salt diſſolv'd in Water, is upon the receſs of the ſuperfluous Liquor, wont to begin its concretion, not as moſt other Salts do, at either the Lateral or Lower parts of the Veſſel, but at the top of the Water, I will not think it impoſſible, that ſometimes in very hot Climates or Weather, the Sea may taſte more ſalt at the top, than at ſome diſtance beneath it. But conſidering how great a proportion of the

Salt common Water is wont to be impregnated with before it suffers Saline Concretions to begin, and how far short of that proportion the Salt contained in the Sea Water is wont to be, insomuch that about *Holland*, a *Dutch* Geographer or two have not found it to amount to the proportion of one to forty, and I in *England* found it to be no more than I shall hereafter specifie; it seems not likely that *Scaliger*'s Observation was well made, and it must be very unlikely that it should generally hold, if the Saltness of the Superficial parts of the Sea be compared with that of the lower parts of it.

See the third Section, towards the latter end.

And yet I do not build my Opinion wholly upon this Argument of some Modern Philosophers, That Salt being a heavier body than Water, must necessarily communicate most Saltness to the lowest parts.

For though this Argument be a probable one, yet Water being a fluid body, the restless agitation of whose Corpuscles makes them and the Corpuscles they carry with them perpetually shift places, whereby the same parts come to be sometimes at the Top, and sometimes at the Bottom. This consideration, together with what was lately noted of the peculiar Disposition of Dissolved Sea Salt, to begin its Coagulation upon the surface of the Water, may make the Argument we are considering suspected not to be so cogent, as at first sight one may think it. Which suspicion I might somewhat countenance by subjoyning, that in divers Metals, and other tinged Solutions, I have not usually observ'd the upper part of the Liquor to be manifestly deeper coloured than the lower; though be-

tween

tween Metalline Bodies and their *Menſtruums*, the diſproportion of ſpecifick gravity does uſually much exceed that which I have met with, between Sea-Salt and Common Water.

CHAP. II.

'Tis urg'd out of *Linſcotten* by a Learned Modern Writer, that wanting freſh Water near *Goa* (the Metropolis of the *Portugals* in the *Eaſt-Indies*) they make their Slaves fetch it, by diving, from the bottom of the Sea, which ſeems a clear evincement of the Peripatetick opinion. But in this Obſervation I cannot acquieſce, for two Reaſons: The one, becauſe that though what is alledged as matter of Fact were ſtrictly true, yet ſo general a concluſion could not be ſafely drawn from that particular inſtance, ſince in other parts of the Sea the contrary has been found by Experience, as I ſhall ſhew ere long. And other reaſons than thoſe given by the Peripateticks may be rendred of what happens at *Goa*, which reaſons may extend to the like caſes, if elſewhere they ſhall happen to be met with. For it may very well be, that Springs of freſh Water may ariſe in ſome parts of the ſurface of the Earth, that are cover'd with the Sea, as they do in innumerable Vallies and other places of the Terreſtrial Surface that is not ſo covered. Not to mention thoſe Springs that appear in divers places upon a low Ebb, cover'd with the Sea during the Flood. The Curious *Hungarian* Governour that gives us an ac-

De Admirandis Hungariæ Aquis.

count

count of the wonderful Waters that ennoble his Countrey, relates, that in the River *Vagus* that runs by the fortress *Galgotium*, the Veins of hot Water spring up in the bottom of the River it self

Neque in Ripa tantùm, sayes he, *eruuntur calidæ, sed etiam intra amnem, si fundum ejus pedibus suffodias; calent autem immodicè*, &c. Nay, I have been assur'd by more than our Learned Eye-witness, that there is a place upon the *Neapolitan* Coast, where they (and I think a Writer or two of those parts) observ'd the Water to spring up hot beneath the Surface of the Sea, insomuch that one of my Relators thrusting in his hand and arm somewhat deeper than was convenient, found there an offensive degree of Heat.

Pag. 65.

Besides, (which is my second conjecture) as to the particular case of *Goa*, I had the curiosity to enquire of a great Traveller, and a man of Letters that liv'd in that City and the neighbouring places, and gave me a pertinent account of them, and especially of that place whence the fresh water is fetch'd by the Divers, which his Curiosity led him to visit, and take special notice of; but I found by him, that the Divers do not now think it needful to fetch their fresh water so low as from the bottom of the Sea, and that by the little depth, whence his and other mens curiosity caus'd it to be taken up, he judg'd it did not so much come from any fresh water Springs rising at the bottom of the Sea, as from a small River (whose name I do not remember) that not far from thence runs into the Sea, with such a juncture of circumstances, that at the mention'd places, the fresh water does yet keep it self

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olerably diſtinct, and is not yet ſo far made brack-ſh, as not to continue potable, though not very ood. Which conjecture of his I could make robable, by what I have had from eminent and bſerving men among our own Navigators, touching he ſliding of Waters one over another, in ſome arts of the Sea, eſpecially near the mouths of Ri-ers. But the diſcuſſion of this matter, and the particulars of the Account given me of the ſcitua-ion of the place where Water is div'd for near *Goa*, would require more words than they would n this place deſerve, unleſs the point under de-ate were more important to our preſent pur-oſe.

I might here pretend to a clear demonſtration y experience of the contrary of what *Scaliger* deli-ers, by vouching the teſtimony of the Learned *Pa-ricius*, who affirms, that being upon the Sea which akes its denomination from the Iſland of *Crete* now *Candia*,) he did, in the company of a *Vene-ian* Magiſtrate, *Moccenigo*, let down a veſſel (fur-niſh'd with a weight to ſink it) to the bottom of he Sea, where, by the help of a contrivance, it was inſtopp'd, and fill'd with Water there, which be-ng drawn up, was found to be not freſh but Salt. This Experiment, I ſay, I could oppoſe as a Demon-ſtration againſt *Scaliger*; but though it be a very robable Argument, and more conſiderable than any I have ſeen brought by the *Peripateticks* for their Opinion, yet I confeſs it would be more ſatisfactory o me, if it would not permit me to ſuſpect, that in he drawing up of the Veſſel through the Salt water, hough there had been Freſh water taken in at the bottom, the taſte may have been alter'd by the

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subingression of Salt water, which being bulk fo bulk heavier than Fresh, would by its ponderousness endeavour to sink into the ascending Vessel. and thereby more easily expell part of the Fresh water, and mingle with the rest. Wherefore I shall confirm the Saltness of the Sea at the bottom by some Observations, that are not liable to the same Objections as that of *Patricius*.

The first is that of the Person, whom I elsewhere mention, to be able by help of an Engine to stay a considerable time at the bottom of the Sea; for of him I learn'd, among other things that I desir'd to be inform'd of touching that place, that he found the Water to have as Salt a taste there as at the top.

The next Observation I obtain'd by means of a great Traveller into the *East* and *West Indies*, who having had the curiosity to visit the famous Pearl-fishing at *Manar*, near the great *Cape* of *Comori*, answer'd me, that he had the same curiosity that I express'd to learn of the Divers, whether they found the Water Salt at the bottom of the Sea whence they fetch their Pearl-fishes? and that he was assur'd by them that it was so: And the same person being asked by me about the Saltness of the Sea in a certain place under the *Torrid Zone*, which the relation of a Traveller inclin'd me to think to abound extraordinarily with Salt, affirm'd to me, that not only the Divers assur'd him, that the Sea was there exceeding Salt at the bottom, but brought up several hard lumps of Salt from thence, whereof the Fishermen and others were wont to make use to season their meat, as he himself also did; which yet I may ascribe not only to the

he plenty of Salt already diſſolv'd in the Water, ut to the greater indiſpoſition, that ſome ſorts of alts, whereof this may be one, have, to be diſolv'd in that Liquor.

To theſe I ſhall add this third Obſervation: Meeting with an inquiſitive Engineer, that had frequented the Sea, and had ſeveral opportunities to nake Obſervations of other kinds in deep Waers, I deſir'd him that he would take along with im a certain Copper Veſſel of mine, furniſh'd vith two Valves opening upwards, and let it down or me the next time he went to Sea; on which ccaſion he told me, that (if I pleaſed) I might ave my ſelf the trouble of the intended tryal, for, vith a Tin Veſſel very little differing from that I eſcribed unto him, he had had the curioſity near he Straight of *Gibraltar*'s mouth, (where he had ccaſion to ſtay a good while) to fetch up Sea-waer from the depth of about forty fathom, and ound it to be as ſalt in taſte as the Water near the Surface.

Theſe Obſervations may ſuffice to ſhew, that the Sea is Salt at the bottom, in thoſe places where hey were made; but yet I thought it was not fit for me to acquieſce in them, but rather endeavour o ſatisfie my ſelf, by the beſt tryal I could procure o be made with my Copper Veſſel, (as more trong and fit than a Tinn one,) what Saltneſs is to be found in the Water at the bottom of our Seas, not only becauſe it may more concern us to know that, but chiefly becauſe, though I deny not, that in the fore-going Obſervations the taſte may ſufficiently prove that the Sea is Salt at the bottom as well as the top, yet I thought the

taſte,

taſte, by reaſon of the prediſpoſitions and other unheeded affections 'tis liable unto, no certain way to judge whether the top and the bottom be as Salt one as the other. Wherefore I thought it would be more ſatisfactory to examine the Sea-water by *weight* than by *taſte*, and in order thereunto, having deliver'd the above-menti-on'd Inſtrument to the Engineer I lately ſpake of, when he was going to Sea, he ſent me, together with it, a couple of Bottles of Sea-water taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour and ſmell of theſe two Waters were ſomewhat differing; but when I examin'd them Hydroſtatically by weighing a roul of Brimſtone firſt in one, and then in the other, I ſcarce found any ſenſible difference at all in their ſpecifick gravities. So that the degree of the Saltneſs of Sea-water may be ſafely determined by its greater or leſſer weight then ſo far forth as this ſingle Experiment inform'd me, the Saltneſs is equal at the top and bottom of the Sea: I ſaid, *if the degree*, &c. becauſe of what I ſhall hereafter take notice of about Salts of leſs ſpecifick gravity than Sea-Salt.

CHAP.

CHAP. III.

IT follows now that I make out, what I formerly intimated, That though it were granted, that ear *Goa*, and perhaps in ſome other places, the Divers may have found the Water freſh at the bot-om of the Sea, it would not therefore neceſſarily ollow, that the Sea-water, generally ſpeaking, is reſh at the bottom; for the Obſervations lately ientioned ſufficiently manifeſt the contrary: And s to thoſe very few places (if really there have een any) where the Sea-water has been found reſh at the very bottom, I think one may aſcribe e taſte of the Water to the bubbling up of Springs Freſh Water, at, or near enough to, thoſe very aces. I know this may appear a Paradox, ſince may ſeem altogether unlikely, that ſo ſmall a ream of Water as can be afforded by a Spring, ould be able to force its way up in ſpite of the iſtance of ſo vaſt a weight as that of the ſuper-cumbent Sea-water, eſpecially ſince this Liquor reaſon of its Saltneſs is heavier *in ſpecie* than reſh Water.

But this Objection needs not oblige me to for-ke my conjecture; for whatever moſt men believe, d even Learned men have taught, to the contrary, matters not how great the quantity of Liquor be, hich is laterally higher than the lower Orifice of e Pipe or Channel that gives paſſage to the Liquor at is to be impell'd up into it; provided the upper rface of the Liquor in the Channel or Pipe have a fficient perpendicular height in reference to that

of

of the stagnant Water; for no more of all this fluid will hinder its ascent, than the weight of such a Pillar of the said fluid as is directly superincumbent on it. *Stevinus* and I have by differing wayes particularly proved, that, according to the Laws of the true Hydrostaticks, the prevalency of two Liquors that press against each other, is not to be determined according to the Quantity of them, but to be adjudg'd to that which exceeds the other in (perpendicular) height; so that considering the Channel wherein a Spring runs into the Sea, as a long and inverted Syphon, if that part of the either neighbouring or more distant shore, whence the Spring or River takes its course, be a neighbouring Hill or Rock, or any other place considerably higher than that part of the bottom of the Sea (or of the shore cover'd with the surface of the Sea) at which the Channel, which conveyes Fresh water, terminates, that Liquor will issue out in spite of the resistance of the Ocean.

V. Stevinum *prop.* 10. *l.* 4. *Statices.* And See the Author's *Hydrostatical Paradoxes.*

To illustrate at once and prove this Paradox, I thought upon the following Experiment. I took a Vessel of a convenient depth, and a Syphon of a proportionable length, both of them of Glass, that their transparency might permit us to see all that passed within them. Into the larger Vessel we put a quantity of Sea-water, and into the longer leg of the Syphon, which had been for that purpose inverted, we poured a convenient quantity of Fresh water, which we kept from running out at the shorter leg, by stopping the Orifice of the longer with the

the thumb or finger: Then this Syphon being ſo plac'd in the greater Veſſel, that the Orifice of the ſhorter leg was a great deal beneath the Surface of the Salt water, and the Superficies of the Freſh water in the longer leg was a pretty deal higher than that of the ſurrounding Salt water, we unſtopped the orifice of the upper leg, whereby the water in the Syphon tending to reduce it ſelf to an *Æquilibrium* (or equality of height) in both legs, the water in the upper leg being much higher and heavier than that in the other, did, by ſubſiding, drive away the Water in the ſhorter leg, and make it ſpring out at the orifice of the ſhorter leg, in ſpite of the breadth and ſpecifick gravity of the Salt water. And this impelling upwards of the Freſh water laſted as long as the ſurface of that water in the longer leg retained its due height above that of the ſurrounding Sea water; which circumſtance I expreſly mention, becauſe there being a difference amounting to between a fortieth and fiftieth part betwixt the ſpecifick gravity of our Sea water and common Freſh water, by reaſon of the Salt, which makes the former the heavier, the Freſh water in the longer leg of the Syphon ought to be between a fortieth and fiftieth part higher than the ſurface of the Sea-water, to maintain the *Æquilibrium* betwixt theſe two Liquors.

To make the fore-mentioned Experiment the more viſible, I thought fit to perform it with Freſh Water ting'd with Braſil or Log-wood; but that it might not be objected, that thereby the ſpecifick gravity of the Liquor would be alter'd or increas'd, I afterwards choſe to make it with Claret Wine, which being a Liquor lighter than Common

Water, and of a conspicuous colour, is very convenient for our purpose.

And when I made this tryal, by placing the Orifice of the shorter leg at a convenient distance below the surface of the Sea-water, 'twas not unpleasant to observe, how upon the removal of the Finger that stopp'd the Orifice of the longer leg, the quick descent of the Wine contain'd in that leg, impell'd the colour'd Liquor in the shorter leg, and made it spring up, at its Orifice, into the incumbent Sea-water, in the form of little red clouds, and sometimes of very slender Streams. And as this shorter leg of the Syphon was rais'd more and more towards the surface of the Water so there issued out more and more Wine at the Orifice of it; the Liquor in the longer leg proportionably subsiding, but yet continuing manifestly higher than the surface of the Salt Water than which it was *in specie* much lighter.

¶ But here I must give an Advertisement to prevent a mistake; for if the Syphon be not exceeding slender, after the Wine in the longer leg is fallen down to its due station, a heedful Observer may perceive after a while, that though the Syphon be kept in the same place, there will issue out of the shorter leg a little red stream, which proceeds not from the former impulse of the Wine in the longer leg, but from the ingress of the Sea-water, which being much heavier *in specie* than Wine, sinks into the Cavity of the Syphon, and as it comes in on one side thrusts up as much Wine on the other side of the same Cavity. But the red Liquor that ascends on this account may be discern'd to do so

by its riſing more ſlowly, and after another manner than that which is impell'd up by the ſudden fall of the tall Cylinder of Wine in the longer leg.

THE SECOND SECTION.

CHAP. I.

AS to the *Cauſe* of the Saltneſs of the Sea, I therein agree with the Learned *Gaſſendus*, and ſome other Modern Writers, That the Sea derives its Saltneſs from the Salt that is diſſolved in it: But I take that Saltneſs to be ſupplied, not only from Rocks, and other Maſſes of Salt, which at the beginning were, or in ſome places may yet be, found either at the bottom of the Sea, or at the ſides, where the Water can reach them, but alſo (to ſay nothing here of what may perhaps be contributed by ſubterraneal Streams) from the Salt, which the Rains, Rivers, and other Waters diſſolve in their paſſage through divers parts of the Earth, and at length carry along with them into the Sea. For not only 'tis manifeſt enough, that ſeveral Countryes afford divers ſalt Springs, and other running Waters, that at length terminate their Courſe in the Sea, but I have ſometimes ſuſpected, that very frequently the Earth it ſelf is impregnated with Corpuſcles, or at leaſt, Rudiments of common Salt, though no ſuch thing be vulgarly taken notice of. Which ſuſpicion may be

confirm'd (to omit what I have elsewhere deliver'd on another occasion) partly by the Observation of some eminent Chymists, who affirm themselves to have found a not inconsiderable quantity of exceeding Saline Liquor upon the evaporation of large quantities of *some* Waters, (for in some *others* I could not find it,) and principally by the quantity of common Salt that is usually found in the refining of Saltpeter; though that be a Salt, which Sir *Francis Bacon*, and other experienc'd Writers teach, that almost every fat Earth kept from the Sun and Rain, and from spending it self in Vegetation, will afford.

a Tract of Subterraneal Menstruums.

But having on another occasion sufficiently shewed, that the Earth does abound with common Salt in many more places than are wont to be taken notice of; and that 'tis probable, that by maturation, or otherwise, Salt may daily grow in the Earth, it will not be necessary to add in this place any thing to what I have said already to prove, that our Common Terrestrial Salt being dissolved, may suffice to make the Sea-water brackish; and the rather, if we call to mind what has been formerly said about the possibility of Springs rising beneath the surface of the Sea, and of Lumps of Salt that were taken up by *Divers*, undissolved, at the Bottom of the Sea; the Ocean may receive supplies of Salt from Rocks and Springs latent in its own Bosome, and unseen even by Philosophers. And this may be one Reason, I conceive, (for I deny not but that there may be others, as the very unequal heat of the Sun, *&c.*) why some Seas are so much Salter than others, or at least, why in some pla

places the Sea-water may be much Salter than in others.

And as we have ſeen, That our common Terreſtrial Salt may be copiouſly enough communicated to the Sea, to impregnate it with as much Saltneſs as we obſerve it to have; ſo I do not ſee, that the difference between that Salt and Sea-ſalt is ſo great, but that it may well be ſuppos'd to be derived from thoſe Changes that the Terreſtrial Salt may be liable to, when it comes into the Sea. For that the Marine Salt and the Terreſtrial do very well agree in the main things, may be argued from the reſemblance both in ſhape, taſte, &c. that may be obſerved between the grains that will be produced, if we expoſe each of them in a diſtinct Glaſs to ſuch a heat, as may ſlowly carry off the ſuperfluous Moiſture, and ſuffer them to coagulate into Cubical or almoſt Cubical Graines: And the leſſer differences that may be met with between theſe two Salts, may well enough be ſuppos'd producible by the plenty of Nitrous, Urinous, and other Saline, to which, in ſome places, may be added, Bituminous bodies, that by Land-floods and otherwiſe are from time to time carried into the Sea, and by ſeveral things that happen to it there, eſpecially by the various agitation it is put into by Tides, Winds, Currents, &c. and (which I would by no means omit) by its being in vaſt quantities expos'd to the Sun and Air.

CHAP. II.

WE may justly be the more careful to determine, whether the Saltness of the Sea-water proceed from Common Salt dissolved in it, because if it appeared to be so, we might the more hopefully attempt to obtain by distillation Sweet water from Sea-water; since, if this Liquor be made by the bare dissolution of Common Salt in the other, 'tis probable, that a separation may be made of them, by such a heat as will easily raise the Aqueous parts of Sea-water, without raising the Saline, whose Distillation requires a vehement Heat, as Chymists well know to their cost. And such a method of separating Fresh water from that which was Salt, would make our Doctrine of use, and be very beneficial to Navigation, and consequently to Mankind. For in long Voyages, 'tis but too common for the makers of them, to be liable to hazards and inconveniencies, for want of Fresh and Sweet water, whereby they are sometimes forced to drink corrupt brackish Water, which gives them divers Diseases, as particularly the Scurvy, and, the usual effect of drinking Salt water, the Dropsie. And Sea-men are wont to receive so many other incommodities by the want of Fresh water, that, to prevent or supply it, they are oftentimes forced to change their course, and sail some hundreds of miles to a Coast, not only out of their way, but unsafe in it self, and perhaps more dangerous, by being infested by Pyrats, or in the hands of Enemies or Savage people; by which meanes they often lose the benefit of their

their *Monſouns*, and much more eaſily other Winds, and frequently their Voyage. And theſe are inconveniencies, which might be in good meaſure prevented, if potable, and at leaſt tolerably wholſome Water, could be obtain'd by Diſtillation, in the midſt of the Sea it ſelf, to ſerve the Sea-men till they could be ſupplied with naturally Freſh water. To make ſome tryals of this, I remember I took ſome *Engliſh* Sea-water, whence I was able to ſeparate betwixt a thirtieth and fortieth part of dry Salt, and having diſtilled it in a glaſs head and body, with a moderate fire, till a conſiderable portion of it was drawn over, we could not diſcern any Saltneſs in it by the taſte; and beſides that I found it ſpecifically lighter than ſuch Water as is daily drunk by Perſons of Quality at *London*, I expos'd it to a more Chymical Examen, and did not by that find any thing of Sea Salt in it, though I have at ſeveral times, by the ſame way, manifeſtly diſcovered a Saltneſs in in-land Waters, that are drunk obviouſly for ſweet Waters. If I would have employed a ſtronger Heat, and Veſſels larger and lower, or otherwiſe better contriv'd for copious Diſtillation, I might in a ſhorter time have obtain'd much more diſtill'd Water; but whether ſuch Liquors will be altogether ſo wholſome, Experience muſt determine. Yet that Sea-water diſtill'd even in no very artificial way, may be ſo far wholſome, as not in haſte to be ſenſibly noxious, but at a pinch uſeful, at leaſt for a while, may be gathered from (what occurrs to me ſince the writing of the laſt Paper) the Teſtimony of that famous Navigator, Sir *R. Hawkins*, who commanded a Fleet in the *Indies* for Queen *Elizabeth*. For he, in the Ju-

dicious Account he gave the World of his Voyage, wherein they were distressed, even in the Admirals ship, for want of Fresh Water, has this memorable passage (as I find it *verbatim* in our diligent *Purchase*.)

In Lib. 7. page 1378. of Purchase; out of Sir R. Hawkins his Voyage.

Although our fresh water had fail'd us many dayes (before we saw the shore) by reason of our long Navigation without touching any Land, and the excessive drinking of the Sick and Diseased (which could not be excused;) yet with an invention I had in my Ship, I easily drew out of the Water of the Sea sufficient quantity of Fresh water, to sustain my people, with little expence of fewel; for with four billets I still'd a hogshead of Water, and therewith dressed meat for the Sick and Whole. The Water so distill'd we found to be wholsome and nourishing.

And because the potableness of Sea-water may concern the Healths and Lives of men, I shall here add, to what I elsewhere deliver about my wayes of examining, whether other waters participate of Salt, two or three Observations I made upon those few distill'd Liquors, I had occasion to draw from Sea-water. Having then upon some of the distill'd Liquor dropt a little oyl of Tartar *per deliquium*, I perceiv'd no clouds at all, or precipitation to be made, whereas a small proportion of that Liquor being dropt into the undistill'd Sea-water it self, it would presently trouble and make it opacous, and, though but slowly, strike down a considerable deal of a whitish substance (which, of what nature it is, I need not here declare;) I found also, that a very small proportion of an Urinous Spirit, such as that of *Sal Armoniac*, would produce a whitish and curl-

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ed ſubſtance (but not a near ſo copious one as the other Liquor) in Sea-water, not yet expos'd to Diſtillation, but not in the Liquor drawn from it: which argued, that there were but few or no ſaline particles of Sea-ſalt aſcended with the Water: For elſe theſe Alcalizate and Urinous Salts would in all likelihood have found them out, and had a viſible operation on them. And I farther remember, that when the Diſtillation was made in Glaſs Veſſels, with an eaſie Fire, not only the firſt running, but the Liquor that came over afterwards, was not perceiv'd to be brackiſh, but good and potable. To which agrees very well, that by a Hydroſtatical Tryal I found our diſtill'd Sea-water to be lighter *in ſpecie* than common Conduit Water, though it exceeded *that* in ſpecifick Levity, leſs than 'twas ſurpaſſed in the ſame quality by diſtill'd Rain-water.

But to return to the Subject whence we have ſomewhat, but, I hope, not uſeleſly, digreſs'd; I know it may be objected, that if the Terreſtrial Salts carried by Springs, Rivers, and Land-floods into the Sea, were the cauſe of its ſaline Taſte, thoſe Waters themſelves muſt be made Salt by it, before they arrive at the Sea. But beſides, that this Objection will not reach the Springs and Rivers of Salt water, that in ſeveral places, either immediately or mediately, diſcharge themſelves into the Sea; it might conclude againſt him that ſhould affirm this imported Saltneſs to be the only cauſe of that of the Sea: But it will not be of force againſt me, who take it to be only a partial cauſe, that by its acceſſion contributes to the degree of Saltneſs we obſerve in the Sea, where this imported Salt

may

may joyn it self with the Salt it finds there already, and being detained by it, contribute to the brinyness of the Water.

If it be urg'd, that from hence it will follow, that the Sea from time to time increases in Saltness, I may suspend my answer till it appear by competent observation that it does not; which, I think, men have not yet made tryals that may warrant them to assert. And if the matter of fact were certain, I think 'twere possible to give a farther answer, and shew probable wayes, how so small an accession of Salt may be dispers'd by nature, and kept from increasing too much.

CHAP. III.

BUt now 'tis seasonable to consider, that the taste of Sea-water is not such a simple saline taste, as Spring-water would receive from *Sal Gemm*, or some other pure Terrestrial Salt dissolved in it, but a bitterish taste, that must be derived from some peculiar cause that Authors are not wont to take notice of. I am not assur'd by any Observations of my own, that this recession from a purely Saline taste is likely to be of the very same kind, and to be equally, or very near equally, met with in all Seas; (not to add a doubt whether it be at all sensible in some.) The cause both of the bitterness and saltness too of the Sea-water, is said to be affirmed by Learned Mr. *Lidiat*, to be adust and bituminous

tuminous Exhalations aſcending out of the Earth into the Sea. But that there is abundance of actual Salt in the Sea-water, to give it its Saline taſte and ponderouſneſs, the Salt, that the Sun does in many places copiouſly ſeparate from the Saltleſs wateriſh parts, ſufficiently manifeſts. But as to the bitteriſh taſte, I think it no eaſie matter to give a true account of it, but am prone to aſcribe it *partly* to the operation of ſome Catholick Agents upon that vaſt body of the Ocean, and *partly* to the Alteration that the Salt receives from the mixture of ſome other things, among which *Bitumen* may be one of the principal.

In the Tract of Subterran. Menſtruums.

But though I have in another Paper ſhewn, that in ſome places of the Sea there are conſiderable quantities of *Bitumen*, or Bituminous matter to be met with; yet I dare not derive the bitterneſs of the Sea only from Bituminous Exhalations, but in good part, at leaſt, in ſome places, from the liquid and other Bitumen, that is imported by Springs and other Waters into the Sea; of which we have an eminent inſtance in that which our *Engliſh* call *Barbadoes* Tar, according to the relation I had of it from an inquiſitive Gentleman, who is one of the chief Planters of the Iſland, and took pleaſure to obſerve this liquid Bitumen to be carried in conſiderable quantities from the Rocks into the Sea; and I think it poſſible enough, that ſome of the Springs that riſe under the ſurface of the Sea, may carry up with them Bituminous matter, which may help to make the Saltneſs of the Sea degenerate, (of which more perhaps elſewhere;) as I not long ſince made mention of Springs, as well of hot

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as cold water, rising beneath the surface of the Sea. And this minds me to intimate here, that I have suspected, that in some places the Sulphureous Exhalations, and other emissions from the submarine parts of the Earth, may sometimes contribute to change the saline taste of the Sea-water: For I have elsewhere related, how not only Sulphureous Steams, but sometimes Actual Flames have broken through from the lower parts of the Sea to the uppermost; and have sometimes taken pleasure to make by Art a rude imitation of that *Phænomenon*. And partly some Experiments of my own, and partly some other Inducements, have perswaded me, that divers times (for I do not say alwayes) Sea Salt does not obscurely participate of Combustible Sulphur, of which I may speak farther on another occasion. But in regard that the taste of the Sea-water is not in all parts of the Ocean uniform, it may here suffice to take notice in general, that this difference of taste may partly be caus'd by adventitious bodies of several kinds, of which 'tis probable, that in differing places the Sea-water does variously partake. And not to mention here the fragrant smell of Violets, which has by several, and particularly by an Eminent Person, of whom I enquired about it, been observed, in some hot Countries, to proceed from Sea Salt; I have divers other Inducements to think that it is usually no simple Salt, nor free from mixture. For by more wayes than one, and particularly by cohobating from it its own Spirit, we have obtained a dry Sublimate, which seemed to be no Pure, but a Compounded Body.

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And now to come to that which I intimated might be one of the cauſes, why the taſte of Sea-water is not the ſame with that of Common Salt diſſolved in Freſh water; I ſhall add, that I have ſuſpected, that the various motion of the Sea, and its being expoſed to the action of the Air and Sun, may contribute to give it a taſte other than Saline; which ſuſpicion might be confirmed by the Obſervation I elſewhere mention of the Sea Salt, which, by barely being expos'd for many months to the Air, and ſometimes perhaps put into a gentle agitation by a digeſtive Heat, I found to have a very manifeſtly differing taſte from the ſimple Solution of Sea Salt in Common water.

I might here endeavour the farther confirmation of my Diſcourſe, by what I have learned by inquiry from Navigators, about the manifeſtly differing Colours, and other Qualities of the differing parts of the Sea, which ſeem to argue, that 'tis not every where of ſuch a Uniform Subſtance as men vulgarly imagined, and that vaſt Tracts of it are imbued with ſtupendious multitudes of adventitious Corpuſcles, which, by ſeveral wayes diverſifying its parts, keep it from being a ſimple Solution of Salt. But of this Subject I have not leiſure to diſcourſe here, only becauſe 'tis generally thought, that the Sea-water is, by reaſon of the Saltneſs it abounds with, uncapable of Putrefaction; I will add, That having kept a pretty quantity of Sea-water, that I had cauſed to be purpoſely taken up between the *Engliſh* and *French* ſhores, in a good new rundlet, in a place where the Summer Sun beat freely upon it, it did, in a few weeks, acquire a ſtrongly ſtinking ſmell; though, that the Experiment had been

been more satisfactory, I wished that it had been made in a Vessel of Glass or Earth, instead of Wood. But a much better Observation I procur'd from a much esteemed Navigator of my acquaintance, who having sailed often in the *Indian* and *African* Seas, I enquired of him, whether he had ever in those hot Climats, where the Sea is supposed to be very Salt, observed it to stink, for want of Agitation or otherwise: To which he answer'd, That once being, though it was but in *March*, becalmed, in a place he named to me, for 12 or 14 dayes, the Sea, for want of motion, and by reason of the Heat, began to stink, insomuch that, he thinks, if the Calm had continued much longer, the stench would have poysoned him: They were freed from it as soon as the Wind began to agitate the Water, and broke the Superficies, which also drove away store of the Sea Tortoises, and a sort of Fish, whose *English* name I know not, that before lay basking themselves on the top of the Water.

And to this agrees very well the notable Observation, that I since met with, of the elsewhere commended Sir *R. Hawkins*, who, among other considerable things he takes notice of in his Relations, has this passage, to our present purpose. *Were it not for the moving of the Sea by the force of Winds, Tides, and Currents, it would corrupt all the world. The Experience I saw* Anno 1590, *lying with a Fleet about the Islands of* Azores, *almost six months, the greatest part of the time we were becalmed; with which all the Sea became so repleniſh'd with several sorts of Gellies, and forms of Serpents, Adders and Snakes, as seem'd wonderful, some*

Purchase's Pilgrims in Sir *R. Hawkins* Observations.

ſome green, ſome black, ſome yellow, ſome white, ſome of divers colours, and many of them had life; and ſome there were a yard and a half, and two yards long, which had I not ſeen, I could hardly have believed. And hereof are witneſſes all the company of the Ships which were then preſent, ſo that hardly a man could draw a bucket of water clear of ſome corruption. In which Voyage, towards the end thereof, many of every Ship fell ſick of this Diſeaſe, and began to dye apace, but that the ſpeedy paſſage into our Country was a remedy to the crazed, and a preſervative for thoſe that were not touched.

THE THIRD SECTION.

CHAP. I.

AS for the various *Degrees* of the *Saltneſs* of the *Sea*, Authors are wont to be ſilent of it, ſave that ſome Navigators tell us, that they obſerved ſome Seas to have a more, and others a leſs Saline taſte; which you will eaſily believe has not afforded me much ſatisfaction. And on the other ſide, my want of opportunity to make Tryals my ſelf, will confine me to acquaint you with no more than the few following Obſervations.

1. To a Learned man that was to ſail to places of differing Latitudes in the *Torrid Zone*, I deliver'd a Glaſs Inſtrument, elſewhere deſcribed, fitted by the greater or leſſer Emerſion of the upper part, to ſhew, accu-

accurately enough for use, the greater or less specifick Gravity of the Salt Water it was put to swim in. This he put from time to time into the Sea-Water, as he sailed towards the *Indies*, whence he wrote me word, *That he found, by the Glass, the Sea-water to increase in weight, the nearer he came to the Line, till he arrived at a certain degree of Latitude, as he remembers, it was about the thirtieth; after which, the Water seemed to retain the same Specifick Gravity, till he came to the* Barbadoes *or* Jamaica.

2. Another Observation I obtain'd by Inquiry of an Ingenious Person and a Scholar, at his return out of the *East Indies*, who affirm'd to me, that he, and a Gentleman of my acquaintance, took up Bottles full of Sea-water, both under the *Equinoctial*, and also off the *Cape of good Hope*, which lies in about 34 Degrees of Southern Latitude; and found the Waters of these distant parts of the Ocean to be of the same weight. And though it may well be doubted, whether this Observation, being made with ordinary Bottles, were so exact as could be wish'd, yet the Persons being curious, and making it for their own satisfaction; and my Relator having, in both the recited places, fill'd with the Sea-water he took up and weigh'd, having, I say, fill'd the *same* Bottles; *since* this Vessel held two quarts, (which must be above four pounds of Salt-water,) *if* the disparity of weight had been *considerable*, it would in likelihood have been found, at least manifestly *sensible* in such a weight of Liquor.

3. Inquiring of an observing Person, that had been at *Mosambique*, which is thought to be one of the hottest places in the World, whether he did not

not there find the Sea to be more than ordinarily Salt; he anſwered me, that, coming thither in a great Carack, when he came back from the Town to the Ship, he obſerv'd near two hands breadth of the Veſſel to be above the ordinary part, to which it uſed to ſink; inſomuch that he took notice of it to the Captain, as fearing that part of the lading had been by ſtealth carried to the ſhore: But the Pilot, who had made thirteen or fourteen Voyages to the Indies, aſſur'd him, what he had obſerved about the Ship was not unuſual in that place, where the taſte it ſelf diſcover'd the Water to be exceeding Salt.

Nor need we ſcruple to think, that ſome Sea-Waters may be very much more impreghated with Salt than ours; for Water will naturally diſſolve, and retain a far greater proportion of Salt, than that which is commonly met with in the Sea. For whereas a thirty fifth, or thirtieth, or at moſt a twenty fifth part of Salt will make Water more Saline than is found in many Seas, I am, by a Friend of mine that is Maſter of a Salt-work, inform'd, that the Water of his Springs afford him a twelfth part of good White Salt, and that another Spring not far off, yields no leſs than an eighth part. To which, (to avoid anticipation) I ſhall not here add, what I ſhall hereafter have occaſion to ſay of the fulleſt impregnation of Water with Common Salt.

[Whilſt I was reviewing theſe Papers, there came ſeaſonably to my hands a Letter written from *Maſilapatan*, on the Gulf of *Bengala* in the *Eaſt-Indies*, by an ingenious Gentleman, Sir *William Langhorn*, that is intruſted with the care of the *Engliſh*

lish Factories in those parts; out of which Letter the following passage is *verbatim* transcribed: "I did, in order to your command, cause some Wa- "ter to be saved under the Line, at our first acces- "to it, intending, for want of good scales and "weights, (being none to be come at aboard the "Ship) to have kept it until it could be weighed; "but by the forgetfulness of a servant, it was thrown "away. Off the Cape, in 37 *d.* 00 *m.* Southern la- "titude, I saved some again, and through the same "want of weights, was fain to keep it until I came "to the Line again; and then made the best shift "could for weights, and compar'd it with the Wa- "ter there, filling the same Bottle again to the same "height by a mark, and found it exactly the same "weight. The weight I have taken; but accounting "this a journey of business, left those notes, and "most of the like nature, behind me; in my next "it shall be inserted.]

CHAP. II.

IT remains now, that, according to my promise I set down what I observed my self concerning the Saltness of our Sea between *England* and *France*; not in comparison with the Saltness of other Seas, whose Waters I had not to compare with, but as to the proportion of Salt contained in it to the Water. And though one would think it very easie to make tryals of this sort for a person no un-

unacquainted with Hydroſtatical practices nor unfurniſhed with Inſtruments, yet, I confeſs, that three or four tryals that I made, not all of them the ſame way, made me find it more difficult than was imagin'd to arrive at any thing of certainty in this inquiry.

This you will eaſily believe, if I annex the ſubſtance of ſome Experiments, that, I remember, I made about the gravity of Sea Water, which I had order'd to be taken up, ſome at the depth of about fifteen Fathom ſomewhat near our ſhoar, and ſome in another place of the Channel between *England* and *France*.

The ſum of the firſt Experiment is this: We took a Vial, fitted with a long and ſtrait neck, purpoſely made for ſuch tryals, and having counterpois'd it, fill'd it to a certain height with common Conduit water: We noted the weight of that Liquor, which being poured out, the Vial was fill'd to the ſame height with Sea Water, taken up at the ſurface, and by the difference between the two weights, the Sea water appear'd to be about a forty fifth part heavier than the other.

The ſecond Tryal (which was for more accurateneſs made Hydroſtatically,) I find regiſter'd to this effect: We carefully counterpois'd in the Scales, formerly made uſe of, a piece of Sulphur in the upper Sea water, formerly mention'd; it weigh'd ℥ß + 10 ½ *gr.* and being alſo weigh'd in the Sea water fetch'd from the bottom, gave us the ſame weight ℥ß + 10 ½ *gr.* which ſhew'd thoſe two Waters to be of the ſame Specifick Gravity: And then to compare this with the gravity of common Water, we weigh'd the ſame Sulphur in com-

mon Conduit Water, and found it $\frac{3}{3}ß + 15\frac{1}{2}$ *gr*: By which it appear'd, that the Sea-water was but about a fifty third part heavier than this Water: which is such a difference from the proportion found out by the former way of tryal, that I could not well imagine what to attribute it to, unless the Sea-water by long standing in a Vessel, which, though cover'd, was expos'd to the hot Sun, may both have been rarified, and have had some separation made of its Saline or other heavier parts, on which score that portion we took up for our tryal, might appear lighter than else it would have done; or unless, the Experiment having been made in *London*, where great and sudden rains and other accidents will sometimes visibly vary the consistence of common Water, the Liquor, I then employ'd without examining it, might be more ponderous at that time than at another. To which latter suspicion I was the more inclin'd, because, having afterwards weigh'd the same piece of Sulphur by help of the same ballance in distill'd rain water, I found the weight of the former liquor to exceed that of the latter by a good deal less than a thirty fifth part; which seem'd to make it probable, that if the Water, we chanc'd to employ, had been free from all Saline and other heavy particles, the difference formerly mention'd betwixt this Observation and the fore-going would not have been near so great as it was.

The last way I made use of to examine the proportion betwixt Sea-water and Fresh, was Chymical; whereof my Register affords me this account.

A pound (*Haverdupois* weight) of the upper Sea-water, was weigh'd out, and put into a head and body

body to be diſtill'd in a digeſtive furnace *ad ſiccitatem*, and the Diſtillation being leiſurely made, the bottom of the glaſs was almoſt cover'd with fair grains of Salt, ſhot into Cubical figures, and more white than was expected; in the reſt of the coagulated matter we took not notice of any determinate ſhape. The Salt being weigh'd amounted to ℥ß, *Haverdupois*, and 10 *gr.* At which rate the proportion of the Salt to the Water will be that of 30 and $\frac{72}{100}$ to one, and ſo will amount to near the thirtieth part; which was ſo much greater than the former wayes of tryal made us expect, that I know not whether it may not be worth while to try, whether ſuch a ſlow abſtraction as we employ of the ſuperfluous Water, and our doing it in cloſe Veſſels, may not have afforded us more Salt than elſe we ſhould have obtain'd.

To this Relation I find this note ſubjoyn'd: Suſpecting that there may have ſomewhat elſe concurr'd to our finding ſo great a proportion of Salt, I ſuffer'd that, which had been weighed, to continue a while in the Scale, and ſoon perceiv'd, that, according to my conjecture, that ſcale began manifeſtly to preponderate, and that conſequently ſome of the unexpected weight of Salt may be due to the moiſture of the Air, imbib'd after the Salt was taken out of the Glaſs, and laid by to be weighed: Wherefore, cauſing it to be very well heated and dried in a Crucible, we found it to weigh ℥iij. + ß. (that is 210 *gr.*) upon which account, the proportion of Salt contain'd in the Water was a thirty ſixth part, and ſomewhat above half of thoſe parts, and to expreſs it in the neareſt whole number, a thirty ſeventh part.

From whence this greater proportion of Salt by Distillation, than our other Tryals invited us to expect, proceeded, seems not so easie to be determined; unless it be supposed (as I have sometimes suspected) that the Operation, the Sea-water was exposed to in Distillation, made some kind of change in it, other and greater than before-hand one would have look'd for; and that, though the grains of Salt we gained out of the Sea-water, seem'd to be dry before we weigh'd it, yet the Saline Corpuscles, upon their concreting into Cubes, did so intercept between them many small particles of Water, as not to suffer them to be driven away by a moderate warmth, and consequently such grains of Salt may have upon this account been less pure and more ponderous than else they would have been. And I might here add, that I sometimes make a certain Artificial Salt, which though being dissolv'd in Water, it will shoot into Crystals finely shaped, and dry enough to be reducible into powder, yet coagulates Water enough with it to make the Water almost, if not quite, as heavy again as before. And I have been assured by a very Learned Eye-witness, that there is a sort of Sea Salt, which they bring to some parts of *England* from the Coast of *Spain* or *Portugal*, which being here dissolved, and reduced by Purification and Filtration to a much whiter Salt, will yield by measure somewhat above two Bushels for one. But to satisfie the scruples and suspicions I could suggest, would require more tryals than I have now time or opportunity to make. What has been already deliver'd, may give at least as scrupulous an account of the Saltness of our *English* Sea-waters, as most other Experimenters would have

have thought it needful to give. And to make a determination with any certainty about the degrees of the Seas Saltneſs *in general*, a great number of Obſervations, made in different Climates and in diſtant parts of the Ocean, would be neceſſary.

CHAP. III.

I Know not whether I may be ſo indulgent to my ſuſpicions as to wiſh, that Obſervations were heedfully made, Whether in the ſame Sea, and about the ſame part of it, the Waters be alwayes equally Salt? For, though that be taken for granted, yet ſince we have no good Obſervations long ſince made to ſilence the ſuſpicion, one may ſuſpect, that, at leaſt in many places, the Saltneſs of the Sea may continually, though but very ſlowly, increaſe by the acceſſion of thoſe Saline Corpuſcles that are imported by Salt-Springs, and thoſe which Rivers and Land-floods do from time to time rob the Earth of. And I ſuſpect it to be not impoſſible, that this or that part of the Sea may be ſometimes extraordinarily, and perhaps ſuddenly, impregnated with an additional Saltneſs from Saline ſteams plentifully aſcending into it, from thoſe Subterraneal Fires, about which I have made it elſewhere probable, that they may burn beneath the bottom of the Sea, and ſometimes ſend forth copious Exhalations into it. But it may prove the more difficult to diſcern

In the Tracts of Subterran. Fires and Steams.

this adventitious Saltness, unless the taste as well as ballance be employed about it; because the Salt, that produces it, may be of such a Nature as to be much lighter *in specie* than common Sea Salt. And the mention of this leads me to give you here the Advertisement I promised you not long ago.

That though the weight of Sea-water be as good a way as is yet employ'd (and better than some others) to determine what Sea-water does most abound in Salt; and though it be possible, that in our Sea, and perhaps in almost all others, this way be not liable to any considerable uncertainty; yet I think it not impossible, that it may sometimes deceive us, especially in very hot Regions; because I have observed, that there may be Volatile Salts, which, though by reason of their activity they make smart impressions on the tongue, and give the water imbued with them a strong Saline taste, yet they add very little, and much less than one would think, to its Specifick gravity: as I have tryed, by Hydrostatically examining Distill'd liquors, abounding in Volatile and Urinous Salts, some of which I found very little heavier than Common Water, and consequently nothing near so much heavier as they would have been made, if they had been brought to so sharp a taste, by having nothing but common Sea Salt dissolved in them: So that, if in any particular place, by any other way, or from the Steams of the Earth beneath, (some of which, I elsewhere shew, may be very analogous to those afforded by *Sal Armoniack*) the Sea should be copiously impregnated with such kind of light Salts, the Sea-water may be much more salt to the taste, and yet be very little heavier. For confirmation of which I find among my notes, that

weigh-

weighing a ſeal'd buble of Glaſs, made heavy by an included Metal, firſt in Spirit of *Sal Armoniack*, that taſted much ſtronger than Sea-water, it weighed ℥iij + 51 ¼ *gr.* and weighing this ſame body in fair Water, it weighed but ℥iij + 45 ¾ *gr.* ſo that notwithſtanding its great Saltneſs, the Spirit was lighter than Common water; though a good part of that comparative Levity may probably be aſcribed to the Liquor wherein the Saline Particles ſwarm, which, by Diſtillation, was grown more defecated and light than Common, though clean, Water.

But for a farther proof, we took a hard lump of *Sal Armoniack*, and though we could not weigh it in Water, becauſe that would have diſſolv'd part of it, yet by a way (I elſewhere teach) I found, that weighing in the ſame Liquor this lump of *Sal Armoniack*, and a lump of good white Sea Salt, (brought me as a Curioſity out of the *Torrid Zone*) the proportion of the latter to a bulk of the Liquor equal to it, was ſomething (though exceeding little) above that of two and a quarter to one, and the proportion of *Sal Armoniack* to as much Water as was equal likewiſe to it, did not above a Centeſm exceed that of one and $\frac{7}{10}$ to one; which falls ſo ſhort of the other proportion as may juſtly ſeem ſtrange, eſpecially if it be conſidered, that the factitious *Sal Armoniack*, the Chymiſts generally uſe, and we employ, conſiſts in good part of Sea Salt, which abates much of the Comparative Levity it might have, if it were made up only of Urinous and Fuliginous Salts, which were its other ingredients.

It were indiſcreet for me to propoſe any more ſuſpicions and tryals fitted to clear them, unleſs I knew

knew those I have already mention'd would not pass for Extravagancies; and therefore I should here dismiss the Subject of this Tract of the Saltness of the Sea, but that since I have been discoursing of the degrees of it, it will not be impertinent to add, what is the greatest measure of Saltness that I have brought Water to, without the help of external Heat. On this occasion I employed two differing wayes, the one was by putting into a well-counterpoysed Vial two Ounces of Common-water, and then putting into it well dryed and white common Salt, and shaking them together till the Liquor would, *whilst cold*, dissolve no more: This Liquor, thus glutted with Salt, weighed 1150 grains, from which two Ounces being deducted, the overplus of weight, arising from the dissolved Salt, amounted to 190 *gr.* so that a parcel of Salt will without heat be dissolved in about five times its weight, or very little more, of common Water. By which proportion we made so strong a brine, that divers pieces of Amber, being purposely let fall into it, emerged, and floated on it. The other and better way, yet more tedious, that we made use of, was, to let Sea-Salt run *per deliquium*, (as the Chymists speak) that is, to set it in some moist place, till it was dissolved by the Aqueous Vapours that swim in the Air. In this Liquor we weighed a piece of Sulphur, which we also weighed in Sea-water, wherein, finding it to weigh much more than in the former Liquor, it appeared that the Sea-water was *in Specie* much lighter than the other; though how much their gravities differ'd, I cannot find among my Notes, nor be informed by my Memory.

And

And becauſe I have not in any Author met with the proportion of Sea Salt to Water of the ſame bulk, nor perceive that Hydroſtaticians themſelves have yet attempted any way to inveſtigate it, (probably deterr'd by the eaſie diſſolubleneſs of Salt in Water) I ſhall here ſubjoyn, that by the help of an Expedient I have elſewhere taught, I have examin'd a hard dry lump of Sea-Salt, and found its proportion in weight to common Water of the ſame bulk, to be *almoſt* as 2 to 1, (for it exceeded the *ratio* of $1\frac{9}{10}$ to 1.) And, I remember, I found the Specifick Weight of a hard and figur'd lump of *Sal Gemm* (which ſort of Salt, I ſuppoſe to be ſomewhat more pure and ponderous than Sea Salt) to be to that of Water (very near) as $2\frac{1}{8}$ to 1.

FINIS.

THE FOURTH SECTION

Belonging to the Tract formerly Publiſh'd under the Title,

Relations about the Bottom *of the* SEA.

By the Honourable *ROBERT BOYLE.*

ADVERTISEMENT TO THE READER.

THis Section should have been subjoyned to the Relations about the Bottom of the Sea, *when that Discourse was printed, together with some other Tracts at* Oxford, An. 1671. *but was by the Negligence of him, that should have carried it to the Press, severed from the rest of that Tract, and not seasonably deliver'd to the Printer.*

THE FOURTH SECTION

Belonging to the Tract intitul'd,

Relations about the Bottom *of the* SEA.

THe presence of the Air is not only so necessary to the Life of many sorts of Animals, but it hath likewise so great a stroke in the growth of Vegetables, especially of the larger sorts, that, after what I had experimented about these matters, (of which this is not the proper place to give an account) I thought fit to make enquiry about the Vegetation and growth of Plants of considerable Bulk in those submarine Regions, where if there grow any, they must do it remote from the free contact of an ambient Air. And having not now the leisure to repeat what *Botanists* (of whose Books I am not now provided) deliver about lesser Plants growing under Water, I shall now onely present you with what information I could procure from Navigators, about Trees and Fruit growing at the bottom of the Sea.

To

To what I have elsewhere had occasion to say to their Opinion, that will not allow Coral to be really a Stony Plant, but a Liveless Concrete, that is alwayes hard and brittle under Water; I shall now add, that, inquiring lately of an Eminent and Inquisitive Person, that had spent some time upon the Coast of *Africa*, where he had been present at the fishing of Coral, and learning from his answer, that he had seen it not far from *Algiers*; I ask'd him, whether he had himself observ'd the Coral to be soft, and not red, when 'twas newly brought from the bottom of the Sea. To which he replied, that he had found it soft and flexible; and that, as for the colour, it was for the most part very pale, but with an eye of red, the Bark being worse coloured than the substance it cover'd was; but when the Bark was taken off, and the other part exposed to the Air, the expected redness of the Coral disclos'd it self.

When I demanded, whether he had observed, that any inky sap ascended to nourish the stony Plant? and whether he had seen any thing like Berries upon it? He ingenuously confessed to me, he had not been so curious as purposely to make inquiry into those Particulars; but that he remembred, That having broken some of the large pieces of Coral, he took notice, that the more internal Substance was much paler than the other, and very whitish, and that at the extream parts of some branches or sprigs he observ'd little blackish knobs, which he did not then know what to make of: And when I enquir'd, what depth the Sea was of in that place? he answer'd, that 'twas nine or ten fathom. But as to the Fruit of some kinds

kinds of Coral, if I do not much misremember, I was, not long since, assured by a Scholar that navigated much in the East, that they divers times meet with in those Seas a certain sort of Coral, but not white, which bears a small Fruit like a round Berry, of a pleasant colour, and esteem'd as rarities.

Discoursing with a person that made *Diving* his Trade, whether he had not met with any Trees or Fruit in the depths of the Sea? He told me, that in a great Ship, whereinto he descended, to recover thence some shipwrack'd Goods, he was surpriz'd to find in several places a certain sort of Fruit, that he knew not what to make of; for he found them of a slimy and soft consistence, about the bigness of Apples, but not so round in shape, and when he brought them up into the Air, as he did many of them, they soon began to shrink up like old rotten Apples, but were much harder, and more shrivel'd. And 'tis remarkable, that this happen'd in a cold Northern Sea.

One that made a considerable stay about *Matar*, a place I have often mention'd, answer'd me, that he learn'd from the *Divers*, that in some places hereabouts there grows at the bottom pretty store of a certain sort of Trees, bearing Leaves almost like those of Laurel, as also a certain Fruit; but of what virtue, or other use, he had not the Curiosity to enquire.

I was also inform'd by an Eye-witness, that near the famous Coast of *Mosambique* in *Africa*, there grows at the Bottom of the Sea store of Trees, that bear a certain Fruit, which he describes

to be very like that, which in *America* they are wont to call *Acayu*, the Leaves also resembling those of that Tree.

But the welcomest Information I could procure about Sub-marine Plants, is that which concerns the famous *Maldivian* Nut, or *Coco*, which is so highly esteem'd in the East, that some write, it is a great Present from one King to another, and even much extoll'd in *Europe* by experienc'd Physicians: For the Origine of this dear Drug is almost as much controverted as the Alexiterial Virtues are extoll'd. Having then once the good fortune to meet with a man of Letters, that had resided in those unfrequented Islands, I found he had been as inquisitive as I could reasonably expect about these admir'd Productions of the Sea, and that he had often learn'd from the *Divers*, that they are real Nuts or Fruits born by a sort of Coco-Trees that grow at the Bottom of the Sea, and are thence either torn off by the agitation of the Water, or gather'd by the *Divers*. These Fruits are smaller than most other sorts of *Coco's*, whose maturity they do not seem to arrive at. He thinks, the *Species* may have been very differing from what it is, and may have come from Nuts fallen into the Sea, together with the ruine of some little Islands undermin'd by the Water, and so submerg'd; of which, he told me, he saw at least three or four instances during his stay there. He told me, that whilst the Fruit was under Water, they observ'd no distinct shell and kernell, but the entire Nut was so soft, that it may be easily enough cut with a knife, and was eaten like their other Fruits; but being kept about a Week

in

in the hot Air, it grows solid, and so hard as to require good Steel Tools to work upon it. He added, that though even upon the place the fairer sort be of very great esteem, yet not of any such prodigious price as is given out. And he presented me one about the bigness of a large Egg, and a Fragment of another, which are both very hard; but as for their Virtues, I can yet say nothing upon Tryal, for want of having had fitting Opportunities.

Other Observations made at the Bottom of the Sea may hereafter follow.

FINIS.

A PARADOX OF THE NATURAL AND PRETERNATURAL STATE OF BODIES,

Especially of the AIR.

By the Honourable *ROBERT BOYLE.*

AN ADVERTISEMENT.

AN attentive Reader will easily be perswaded by a couple of passages in the following Papers that it is only a Fragment. But though the Author, for certain Reasons, has for divers years suppress'd the other Discourses that belong to the same Treatise, yet he was content to let this come abroad without them; not only because, relating chiefly to the Air, it may fitly be consorted with those Papers concerning some Qualities of the Air, which it accompanies; but because 'tis hop'd, it may prevent, or put an end to, several unnecessary Disputes about the Natural and Forc'd Constitution of the Air (warmly agitated among Learned men,) by shewing them to be founded, some upon precarious suppositions, and more upon Vulgar Mistakes.

OF THE NATURAL AND PRETERNATURAL STATE OF BODIES,

Especially the AIR.

I Know, that not only in Living, but even in Inanimate, Bodies, of which alone I here discourse, men have universally admitted the famous Distinction between the *Natural* and *Preternatural* or Violent state of Bodies, and do daily, without the least scruple, found upon it *Hypotheses* and Ratiocinations, as if it were most certain, that (what they call *Nature*) had purposely framed Bodies in such a determinate state, and were alwayes watchful that they should not by any external Violence be put out of it.

But notwithstanding so general a consent of men in this point, I confess, I cannot yet be satisfied about it in the sence wherein it is wont to be taken. 'Tis not that I believe, that there is *no sence*, in which, or in the account upon which, a Body may be said to be in its *natural* state; but that I think the *common* Distinction of a *natural* and *violent* state of Bodies has not been clearly explained, and considerately setled, and both is not well grounded, and is oftentimes ill applyed. For, when I consider, that whatever state a Body be put into, or kept in, it obtains or retains that state according to the Catholick Laws of Nature, I cannot think it fit to deny, that, in this sence, the Body propos'd is in a *natural* state; but then, upon the same ground 'twill be hard to deny, but that those Bodies, which are said to be in a *violent* state, may also be in a *natural* one, since the violence, they are presumed to suffer from outward Agents, is likewise exercised no otherwise than according to the established Laws of Universal Nature. 'Tis true, that when men look upon a Body as in a *preternatural* state, they have an *idea* of it differing from that which they had whilst they believ'd it to be in a *natural* state: But perhaps this difference arises chiefly from hence, that they do not consider the condition of the Body, as it results from the Catholick Laws setled among things Corporeal, and relates to the Universe, but estimate it with reference to what they suppose is convenient or inconvenient for the particular Body it self. But however it seems to me, that mens determining a Body to be in a natural or preternatural state has much more in it, either of casual, or of arbitrary, or both, than they are aware of. For oftentimes we think

think a Body to be brought into a violent ſtate, not becauſe really the former was not ſo, but becauſe there is a notable change made in it by ſome Agent, which we alſo take notice of; whereas before the action of that Agent, if the Body were under any violence, 'twas exercis'd by uſual, but often immanifeſt Agents, though perhaps their Compulſion were not *leſs*, but only leſs *heeded*. And ſometimes alſo no more is to be underſtood by a Bodies being forc'd from its Natural ſtate, than that it has loſt that, which it had immediately, or a pretty while before ſome notable change. Which Conjectures I ſhall now endeavour to confirm, but with great brevity.

I have already ſhewn, that Matter being devoid of ſenſe and appetite, cannot be truly and properly ſaid to *Affect* one ſtate or condition more than another, and conſequently has no true deſire to continue in any one ſtate, or to recover it when once loſt; and Inanimate bodies are ſuch, and in ſuch a ſtate, not as the material parts they conſiſt of, elected or deſired to make them, but as the natural Agents, that brought together and rang'd thoſe parts, actually made them. As a piece of Wax is unconcern'd, whether you give it the ſhape of a Sphere, or a Cone, or a Pillar, or a Boat; and whether, when it has that form, you change it into any other; the matter ſtill retaining without willingneſs or unwillingneſs, becauſe without perception, that figure or ſtate which the laſt action of the Agents (your fingers or inſtruments) determined it to, and left it in.

But this will be beſt underſtood, as well as confirmed, by particular examples. I need not tell you, that

that the most usual instance alledg'd to shew, that a state is natural to a Body, and that being put out of it by external causes it will upon the cessation of their violence be restored thereunto, is, That Water being heated by the Fire, as soon as that adventitious Heat vanishes, returns to its native coldness; and so when, by an excess of Cold, it is congeal'd into Ice, it does upon a thaw lose that preternatural hardness, and recover the fluidity that naturally belongs to it: And the same may be likewise said of Butter, which, being melted by external Heat into a Liquor, does upon the cessation of that Heat grow a consistent body again. But perhaps these instances will rather countenance our *Paradox* than disprove it. For as to the coldness whereto Water heated by the Fire returns when 'tis remov'd thence, it may be said, that the acquired Heat consisting but in the various and brisk agitation of the Corpuscles of the Water by an external agent, it need be no wonder, that when that Agent ceases to operate, the effect of its operation should cease too, and the water be left in its former condition, *whether* we suppose it to have been heated by the actual pervasion of the Corpuscles of the Fire, which by degrees fly away into the Air; *or* that the Heat proceeds from an agitation imparted by the Fire to the Aqueous Corpuscles, which must by degrees lose that new agitation, by communicating it little by little to the contiguous Air and Vessel; so that, if the former agitation of the particles of the Water, were, as is usual, much more languid than that of our Organs of Feeling, in which faintness of motion the coldness of Water consisted, there will be no need of any positive internal form,

form, or any care of Nature to account for the Waters growing cold again. This will be confirm'd by the consideration of what happens to Ice, which is said to be Water brought into a preternatural state by an excess of Cold. For, I doubt, 'twill not be easily demonstrated, that in reference to the nature of things, and not to our arbitrary *ideas* of them, *Ice* is Water preternaturally harden'd by Cold, and not Water Ice preternaturally thaw'd by Heat. For if you urge, that Ice left to it self will, when the Frigorifick agents are removed, return to Water; I shall readily answer, that, not to mention the Snow and Ice that lyes all the Summer long unthawed upon the tops of the Alps and other high mountains, I have learn'd, by inquiry purposely made, from a Doctor of Physick, who for divers years practised in *Muscovy*, that in some parts of *Siberia* (a large Province belonging to the *Russian* Emperour) the surface of the ground continues more Months of the year frozen, by what is call'd the natural Temperature of the Climate, than thaw'd by the Heat of the Sun; and that a little beneath the surface of the ground, the Water, that chances to be lodged in the cavities of the Soil, continues frozen all the year; so that, when in the heat of Summer the Fields are covered with Corn, if then you dig a foot or two, perhaps less, you shall easily find Ice and a frozen Soil: So that a man born and bred in the inland part of that Country, and inform'd only by his own Observation, may probably look upon Water as Ice violently melted by that Celestial Fire, the Sun, whose heat is there so vehement in their short Summer, as to ripen their Harvest in less time than in our Temperate Climates will easily be credited.

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On the other side we in *England* look upon melted Butter, as brought into a violent state by the Operation of the Fire, and therefore think, that when being remov'd from the Fire it becomes a consistent Body again, it has but recover'd its Native Constitution. Whereas there are divers parts of the *East Indies*, and, I doubt not, of other hot Countryes, whose Inhabitants, if they should see consistent Butter (as sometimes by the care and industry of the *Europeans* they may do) they would think it to be brought to a preternatural state, by some artificial way of Refrigeration. For in those parts of the *Indies* I speak of, (though not in all others) the constant temper of the Air being capable to entertain as much of agitation as suffices for fluidity in the parts of what in our Climate would be Butter, 'twould be in vain to expect, that, by being left to it self in the Air, it should become a consistent Body. And I have learn'd by diligent inquiry of Sea-men and Travellers, both *English* and others, that were Eye-witnesses of what they told me, that, in divers parts of those hot Regions, Butter, unless by the *Europeans* or their disciples purposely made in the Cold, is all the year fluid, and sold, or dispens'd, not as consistent Bodies, by weight, but as Liquors, by measure. To strengthen this Observation, I shall add, what was affirm'd to me by a Learned man, that practis'd Physick in the warmer parts of *America*, namely, that he met in some places with several Druggs, which, though they there seem to be Balsoms, as Turpentine, *&c.* are with us, and retain'd that consistence in those Climates, yet when they come into our colder Regions, harden into Gums, and continue such both

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Winter and Summer. On the other side, inquiring also of a Traveller, vers'd in Physical things, about the Effects of great Heat in the in-land part of *Africa*, where he had lately been; he told me, among other things, that Raisin of *Jalap*, which, when he carried it out of *England*, was of a consistence not only dry but brittle, did, when, and a while before, he came to *Morocco*, melt into a substance like Turpentine; so that some of it that he had made up into Pills, would no more at all retain that shape, but remain as it were melted all the while he stayed in that City, and the neighbouring Countrey, though when he came back to the borders of *Spain*, it return'd to its former consistence. Which I the less wonder'd at, because, having had the curiosity to consider some parcels of Gum *Lacca*, (of which Sealing Wax is made) newly brought ashore from the *East Indies*, though it be a hard and solid Gum, yet I found by several instances, that, passing through the *Torrid Zone*, divers pieces of it, notwithstanding the shelter afforded it by the great Ship it came in, had been, by the Heat of the Climate, melted, and made to stick together, though afterwards they regain'd their former Consistence, though not altogether their former Colour. And on this occasion I shall add, that I learn'd by inquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place where 'twas made, among other things, furnished himself with a quantity of the best *Aloes*, he observed, that whilst he sail'd through very hot Climates, it was so soft, that, like liquid Pitch, it would often have fallen out of the wide-mouth'd Vessel he kept it in, if he had not from

time

time to time been careful to prevent it. But when he came within a hundred Leagues of the Coast of *England*, it grew hard, and so continued, though this were in a very warm season of the year, being about the Dog-dayes.

For further confirmation of what has been hitherto discoursed, be pleased to consider with me that most obvious Body, the *Air*, or the Atmosphere we live and breath in. For though several Opinions and Argumentations are founded upon what their Authors call the Natural and Preternatural or Violent state of the Air, yet he that considers, shall find it no easie thing to determine, what state of the Air ought to be reputed its truly Natural state, unless in the sence I formerly told you I employ that expression in. I will not insist on the Heat and Coldness of the Air; for, that being manifestly very differing in the heart of Winter, and in the heat of Summer, and in differing Regions of the Air, as at the top and bottom of high mountains, at the same time, and constantly in differing Regions of the Earth, as in *Barbary* and *Greenland*, 'twill not be so easie to determine what state is natural to the Air. But that only which I shall now consider, is its state or tone in reference to *Rarity* and *Density*. For, since the Air is believed to be condensed by Cold and expanded by Heat, I demand, at what time of the year, and in what Countrey, the Air shall be reputed to be in its *Natural state?* For, if you name any one time, as the Winter, or the Summer, I will ask, why that must be the standard of the tone of the Air rather than another Season, or at least exclusively to all others? And the like difficulty may be made about the Cli-

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mate or the Place. And these scruples are the more allowable to be propos'd, because Learned men have deliver'd, that in some Countryes the *Mercury* in the *Torricellian* Experiment, is kept higher than in others, (as in *Sweden* than in *Italy*,) and our Baroscopes inform us, that oftentimes, in the same place and day, the Quick-silver in the same Instrument does considerably vary its height; which shews, that the Air or Atmosphere must necessarily vary its weight, and therefore probably its degree of Rarity or Density.

But I have yet to propose a further Consideration in this Affair: For, what if it shall appear, that neither in Winter nor in Summer, in *Sweden* or in *Italy*, or in whatever Country, Region, or Season you please, the Air we breath in is in any other than a Preternatural state; nay, that even when we have vehemently agitated and expanded it by an intense heat of the Fire, it is not yet violently rarified, but yet violently constipated, unless in our sence before declared, you understand with me the Preternatural state of Rarefaction in the Air, in reference to the tone it had before the last notable change was produc'd in it. This will, I question not, seem a surprizing, if not a wild, Paradox: But yet to make it probable, I shall only desire you to reflect upon two or three of my *Physico-Mechanical* Experiments; and there you will see, *first*, that the Air being a Body abounding with springy particles, not devoid of Gravity, the inferiour must be compress'd by the weight of all the incumbent. And *next*, that this Compression is so great, that though by the heat of the Fire, neither others nor we could bring a portion of included Air to be ex-

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panded to above fourſcore times its former ſpace; yet without heat, by barely taking off the preſſure of the ſuperiour Air, by the help of our Pneumatical Engine, the Air was rarified more than twice as much: And ſince thoſe Experiments were publiſhed, I more than once rarified it to above five hundred times its uſual Dimenſions; ſo that, if according to what is generally agreed on and taught, a Body be then in a Preternatural ſtate, when by an external force it is kept in a condition, from which it inceſſantly tends to get free; and if it be then moſt near its Natural ſtate, when it has the moſt proſperouſly endeavoured to free it ſelf from external force, and comply with its never-ceaſing tendency; if this be ſo, I ſay, then the Air we live in is conſtantly in a Preternatural ſtate of Compreſſion by External force. And when it is moſt of all rarified by the Fire, or by our Engine, its Springs having then far more conveniency than before to diſplay themſelves, which they continually tend to do, it anſwerably approaches to its Natural ſtate, which is to be yet leſs compreſſed or not at all. And I have carefully try'd for many months together, that when the Air has been rarified much more than even a vehement heat will bring it to be, yet if it were fenc'd from the preſſure of the external Air, it would not ſhrink to its former dimenſions, as if it had been put into a violent ſtate, from whence Nature would reduce it to them, but continued in that great and ſeemingly preternatural degree of extenſion, as long as I had occaſion to obſerve it. One might here ſhew, that this odd conſtitution of the Air is ſo expedient, if not neceſſary for the Motion, Reſpiration, and other uſes of Animals, and in par-

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ticular of men, that the Providence and Goodness of the Wise Author of the Universe is thereby signally declared; if it were not improper in such a Paper as this to imploy *final* Causes. Wherefore to avoid the imputation of impertinence, I will conclude, by taking notice that from what has been delivered we may learn two things considerable enough, if not in themselves, yet to some passages of the Treatise, whereof this Paper makes a part. And first, we may deduce from what has been said of the Air, that according to what is noted above, That may sometimes generally be granted and believ'd to be the Natural state of a Body, not which it really affects to be in, or (to speak more properly) has a tendency to attain, but that which it's brought into and kept in by the action or resistance of neighbouring Bodies, or by such a concourse of Agents and Causes as will not suffer it to pass into another state. And the second thing we may hence learn is, that whatever men say of Natures never missing her aim, and that nothing violent is durable; yet, bating an inconsiderable Portion of Aerial particles at the upper surface, for ought we know the whole mass of the Air we live in, and which invirons the whole Terraqueous Globe, has been from the worlds beginning, and will be to its end, kept in a state of violent Compression.

FINIS.

A STATICAL

Hygroscope

Proposed to be further tryed,

Together with

A BRIEF ACCOUNT

OF THE

Utilities of HYGROSCOPES.

By the Honourable *ROBERT BOYLE*,
Fellow of the *Royal Society*.

LONDON,
Printed by *E. F.* for *R. Davis*, Bookseller
in *Oxford*, 1673.

A STATICAL HYGROSCOPE

Proposed to be farther tryed,

In a Letter to *H. Oldenburgh* Esq;

Secretary to the ROYAL SOCIETY.

SIR,

THough I writ to you from *Stanton* an account of those Hygroscopes, whereof I now present you one; yet, since I remember that it was in the year 1665 that I sent you that Paper, I fear you may by this time have forgotten much of what it contain'd, and hereby made it fit for me in this Letter, both to remind you of some former passages, and to add some Observations that lately occurr'd to me; and this the rather, because I do not present you with this trifle meerly to gratifie your Curiosity, but that you

and some of your ingenious friends may, by your remarks, help me to discover to what inconveniences our Instrument is liable, how far they may be avoided or lessen'd, or what the uses or advantages of it may be, notwithstanding its inevitable inconveniences or imperfections.

Having had occasion amongst other subjects relating to the Air, to consider its Moisture, and its Dryness, I easily discern'd that they had no small influence upon divers Bodies; and among the rest, upon those of men, as the ambient Air we breath in, either passes from one of those Qualities to the other, or even from one degree to the other in the same quality.

Wherefore I began to cast about somewhat sollicitously for a way that might better than any I had yet tryed, or elsewhere met with, discover the changes of the Air as to moisture and dryness, and the degrees of either quality. For which purpose it seem'd to me, that, if a Statical Hygroscope could be had, it would be very convenient, in regard of its fitness, both to determine the degrees of the moisture or dryness of the Air, and to transmit the Observations made of them to others. Whereupon considering further, that among Bodies otherwise well qualified for such a purpose, that was likeliest to give the sensiblest informations of the changes of the Air, which, in respect of its bulk, had the most of its surface exposed thereunto; I quickly pitch'd upon a fine Spunge, as that which is easily portable, not easie to be divided or dissipated, which, by its readiness to soak in Water, seem'd likely to imbibe the Aqueous particles that it may meet with dispers'd

spers'd in the Air, and which, by its great porousness throughout, has much more of *Superficies* in reference to its bulk, than any Body not otherwise less fit for the intended use that came into my thoughts.

If you recall to mind, when and whence I first gave you notice that I employed our little instrument, you will easily believe, that the Inducements I had to pitch upon it, were, that I should need but such light and parable things, as I could easily both procure in the Country (where I then was) and carry about with me in the frequent removes I was obliged to make; and therefore that I did not represent this trifle as the best Hygroscope that could be devised, or even as the best that perhaps I my self could have propounded, if I would have fram'd an elaborate Engine with Wheels, Springs, or equivalent Weights, Pullies, *Indices*, and other contrivances, some of which I divers years ago made use of. For I little doubt, but that Mechanical heads may frame Hygroscopes much curiouser and perfecter than that I now send you, or any other I have used or seen, if they may be accommodated with sufficient room, and dextrous Artificers that will work exactly according to directions; whereas my design being not so much to make a *Machinal* or Engine-like, as a *Statical*, *Hygroscope*, and such an one as may be simple, cheap, contained and set up in a little room, easie to be made and transported, I thought it might be of some use, especially to those that are not furnished with Curiosities and Mechanical Accommodations, if among the several forms of Hygroscopes that I had in my mind, I chose one, that being *statical* and easie, might be as

commodious by its simplicity, as some others by their elaborateness; especially if we consider, that, as slight an instrument as it seems, it may be applyed to various uses, some of which are not slight, as will ere long be made probable.

If I should be here told by one, that grants the preferableness of Statical Hygroscopes in the general, that there are *divers Bodies*, other than that pitch'd upon by me, whose weight may vary when the Temperature of the Air is considerably altered as to dryness and moisture, and that perhaps among these, some one may be found that may imbibe the Aqueous particles of the Air better than our Spunge; I shall not resolutely deny it, and therefore shall leave you to make tryals with what other Bodies you shall think fit, contenting my self to have suggested in general the conveniency of making Hygroscopes, where the differing changes of the Air may be estimated by weight; but this I shall tell you in favour of our Spunge, that when I was considering, what Bodies were the fittest to be employed for the making of Statical Hygroscopes, I made tryal of more than one that seem'd not the least promising. I know, that Common or Sea-Salt will much relent in moist Air, and Salt of Tartar will do it much more; but then those Salts, especially the latter, will not so easily as they should, part with the Aqueous Corpuscles they have once imbibed, and are in other regards, (which 'twere not worth while to insist on,) less convenient than a Spunge. I made tryal also with Lute-strings, which were purposely chosen very slender, that they might have the greater surface in respect of

their

their bulk; these I found at first to do very well, as to the imbibing of the moisture of the Air, but afterwards they did not continue to answer my expectation. I caus'd likewise to be turn'd out of a light wood a Cup, which, that it might less burden a tender balance, had, instead of a foot, a little button, to which a hair might be tied, to suspend it by; and this Cup being purposely turn'd very thin, that it might have much surface exposed to the Air, proved for a pretty while so good a Hygroscope, as invited me to make divers Observations with it, some of the which I have still by me. It agreed also with several tryals, that I had made on other occasions, of the porousness of such Bodies, that white Sheeps Leather, such as Chirurgeons us'd to spread plaisters upon, would be very convenient for my purpose. And indeed I found by many Observations, whose success you may command a sight of, that if this Leather were a substance as little obnoxious to Corruption as a Spunge, it would, by its copious imbibitions and emissions of the Aerial moisture, be a fitter matter than any other I had employ'd for a Hygroscope.

But taking all things together, I found no Body so convenient for my purpose as a Spunge, which you will perhaps the more easily believe, if I add, that to help me to make some estimate of the porosity of it, [We weigh'd out a dram of fine Spunge, and having suffer'd it to soak up what Water it could, it was held in the Air, not only whilst the weight of the Water would easily make it run out, but till it dropt so very

slowly,

slowly, that a hundred was reckon'd after one drop before another fell; then putting it into the balance it had been weighed in before, we found, that as its dimensions were increased to the Eye, so its weight was increas'd upon the scale, amounting now to somewhat above two Ounces and two Drams; so that one Dram of Spunge, though it seem'd not altogether so fine as the portion we had chosen out for our Hygroscopes, did imbibe and retain seventeen times its weight of Water.]

Now when one is resolved to employ a Spunge, there will not need to be much added about the turning it into a *Hygroscope*. For, having weigh'd it when the Air is of a moderate Temperature, it requires but to be put into one of the scales of a good balance suspended on a Gibbet (as they call it) or some other fix'd and stable supporter. For the Spunge being carefully counterpoised at first with a metalline weight (because that alters not sensibly with the changes of the Air) it will by its decrement or increase of weight shew, how much the neighbouring Air is grown dryer or moister in the place where the instrument is kept. The weight of the Spunge may be greater or less according to the bigness and goodness of the balance, and the accurateness you desire in the discoveries it is to make you. For my part, though I have for Curiosities sake with very tender scales imployed for a good while but half a dram of Spunge, and I found it to answer my expectation well enough; and though, when I us'd a

bulk

bulk divers times as great, in a ſtronger, but proportionably leſs accurate, balance, I found not the Experiment ſucceſsleſs; yet after tryals with differing quantities of Spunge, I preferr'd, both to a greater and leſſer weight, that of a dram, as not being heavy enough to overburden the finer ſort of Goldſmiths ſcales, and yet great enough to diſcover changes conſiderably minute, ſince they would turn *diſcernably* with a ſixteenth or twentieth part, and *manifeſtly* with half a quarter of a grain.

With ſuch *Hygroſcopes* as theſe (wherein the balance ought to be ſtill kept ſuſpended and charged) I made ſeveral tryals, as my removes and accommodations would permit, ſometimes in the Spring, and ſometimes in the Autumn, and ſometimes alſo in the Summer and Winter. But nevertheleſs it would be very welcome to me, if you and ſome of your Friends would be pleaſed to make tryals your ſelves, and compare them with mine, and eſpecially take notice, if you can, whether in any reaſonable tract of Time there will be any loſs (worth noting) of the ſubſtance of the Spunge it ſelf; I having not hitherto diſcover'd any. In the mean time, to invite you to give your ſelves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a dram of Spunge ſuſpended during a whole Spring, and a great part of the preceding Winter and ſubſequent Summer, I did not think my pains loſt, though divers of the obſervations they afforded me have unhappily been ſo, among many

ny other memorials about Experiments of differing kinds; notwithstanding which unseasonable loss I shall venture to suggest some things to you, that occurr'd to me about the *Utilities* of the Instruments I am treating of.

A BRIEF

A BRIEF ACCOUNT OF THE Utilities of Hygroscopes.

THe use of a Hygroscope is either general or particular: The former is almost coincident with the Qualifications to be wished for and aim'd at in the Instrument it self; The latter points out the particular applications that may be made of it when 'tis duely qualified. Of each of these I shall briefly subjoyn what readily occurrs to me.

The general use of a Hygroscope is, *To estimate the changes of the Air, as to moisture and dryness, by wayes of measuring them, easie to be known, provided, and communicated.*

I might here pretend, That as these are the principal things that have been desir'd in Hygroscopes, so 'tis obvious from the description and account we have

have given of our Instrument, that these advantages belong to it in no very despicable degree: And that to make such Hygroscopes as will perform all these things in perfection, whatever it may seem to a Mental contriver, will, I fear, prove no easie task to those that really attempt it. To these things I might add, that if such allowances be made, as what I have represented may invite you to grant, the Qualifications lately mentioned, as desirable in a Hygroscope, may in a tolerable measure be found in ours, when we shall come to mention the particular uses of it. And as for that of conveying to others the Observations made with it, you may please to consider, that the things I employ to measure the degrees of dryness and moisture in the Air, being grains, parts of grains, and greater weights, the accessions of moisture which the Spunge receives, or the losses that it suffers, can be easily and at the same time both found and determin'd. And as the weights imployed to determine these differences are easily procurable; so the Observations made with them, may (together with patterns, if it should be needful, of the weights themselves) with the same facility be communicated by Letters even to remote parts. In which conveniency, whether, and how far, our Instrument has the advantage of that made with an *Oaten beard*, and some others that I have imployed, I leave you to consider.

I might farther alledge on the behalf of our Instrument, that whereas, besides the Qualifications above mentioned, there is another, namely Durableness, which though not so necessary to constitute a Hygroscope, yet is necessary, as will ere long appear, to some of the considerablest uses of it:

And

And *whereas* ſuch a Durableneſs is wiſhed, as may not only keep the Inſtrument from having its ſubſtance rotted or corrupted by the Air, but may alſo preſerve it in a capacity to continue pretty uniformly its Informations of the Air's moiſture, even when that increaſes very much, or laſts very long; *Whereas*, I ſay, theſe things are much deſir'd in a Hygroſcope, our Spunge ſeems herein preferrable to the Oaten beard, Lute-ſtrings, *&c.* For in thoſe and the like Bodies the ſelf-contracting or relaxing power (as 'tis ſuppos'd) or the diſpoſition to imbibe and part with the moiſture of the Air uniformly or after a due manner, is wont to be in no very long time alter'd or impaired; and particularly, when they have imbib'd much aerial moiſture, they are very faintly affected by the ſupervening degrees of it, and ſo the operation is too diſproportionate to what the like Cauſe would have produc'd, when the Inſtrument was well diſpos'd; whereas in our Spunge neither the degree of ſpringineſs, nor any ſuch like quality is conſider'd, and it is capable of imbibing ſo much more of the Aqueous particles, than even moiſt Airs and Seaſons are wont to ſupply it with, that there is little fear that it will be glutted, or have its pores choaked up with them, ſo that the decrements and acceſſions of weight will be more proportionate to the degree of moiſture in the Air, and more reducible to known and determinate meaſures.

But though theſe and the like ſpecious things may be repreſented in favour of our Statical Hygroſcope; yet, to deal ingenuouſly with you, I much fear, that 'twill be very difficult to bring either Statical ones, or perhaps any other, to be ſo compleat as to ſatisfie

fie a nice and severe Critick. And you would perhaps easily assent to my Opinion, if it were not too tedious to entertain you with all the speculative doubts and scruples, as well Mechanical as Physical, which my accustom'd diffidence has now and then suggested to me. But because such a sceptical Discourse would be too tedious, and also somewhat improper, to be propos'd by one that would recommend Hygroscopes, I shall only now take notice of one great Imperfection, which all that I have been acquainted with are liable to; namely, that men have not yet found, nor perhaps so much as dream'd of seeking, a Standard of the Dryness and Moisture of the Air, by relation to which, Hygrometers may at first be adjusted, and so be compar'd with one another, as we see many of those seal'd Thermoscopes, that have been made and justn'd by Mr. *Shotgrave* the dextrous Operator of the *Royal Society*. I deny not, that, by virtue of a standard to estimate moisture by, I have endeavour'd to remedy this inconvenience; but, as my hopes were but small, so neither was my success great, but I am not sure, that happier Wits, or I my self at some other and luckier time, may not more prosperously attempt it. In the mean while perchance you will not think it altogether nothing, if the Trifle I present you perform at least some of the things desir'd in a Hygrometer less imperfectly, than any you have yet met with. And that you may not be discourag'd by what I have lately acknowledg'd of the defects of such Instruments; I think it now seasonable to proceed to the mention of the particular *Uses*, for which, notwithstanding any inevita-

evitable defects, a Hygroscope, and even such a one as I now present you, may be made easily to serve.

USE I.

To know the differing Variations of Weather in the same Month, Day and Hour.

IT may be useful for divers purposes, to know both that the Air is wont to be less moist at one part of the Artificial Day (and so of the Night,) than at any other, & at what particular time of the Day or Night it most usually is so. And on this occasion I remember, that usually when the Weather was at a stand, it was observed, that the Spunge had manifestly gain'd in the Night, though it were kept in a Bed-chamber, and grew lighter again between the morning and noon. This Observation which was made towards the end of Winter would not hold, in case frosty nights or some other powerful Cause intervened. 'Twere not amiss also to observe, Whether there be not a Correspondence betwixt the Hygroscope and Baroscope; and, if there be, in what kind of Weather or Constitution of Air it is most or least to be discerned. And this enquiry seems the more dubious, because the same changes of the Atmosphere may, upon differing accounts, have either the like, or quite contrary, operations upon these two Instruments. For in Summer when the Atmosphere is usually heavier, the Hygroscope is

usually lighter; some strong Winds, as with us the North-west, may make both the Atmosphere and Baroscope lighter, whereas Southerly Winds, especially if accompanied with rain, often make the Atmosphere lighter and the Spunge heavier. And on the other side I observe, that Easterly Winds, especially when they begin to blow in Winter, though, by reason of their dryness, they are wont to make the Hygroscope lighter, yet they are wont, at least here at the West-end of *London*, to make the Baroscope shew the Air to be heavier. It were likewise fit to be observed particularly by those that live on the Sea-coast, Whether the daily ebbing or flowing of the Sea, do not sensibly alter the weight of the Hygroscope. It were very well worth while also to take notice, at what time of the day or night, *cæteris paribus*, the Air is the most damp and most dry, and not only in several parts of the same day, but in several dayes of the same month; especially on those days, wherein the full and new Moons happen. And this seems a more hopeful way of discovering, whether the full Moon diffuses a moisture in the Air, than those Vulgar Traditions of the plumpness of Oysters and Shell-fish, and brains in the heads of some Animals, and of Marrow in their bones, and divers other *Phænomena*, which, as I have shewn in another paper, 'tis not easie to be sure of. It may also be noted, whether Monthly Spring-tides, especially when they fall out near the middle of *March* or *September*, have any sensible operation upon our Instrument.

USE

USE II.

To know how much one Year and Season is dryer or moister than another.

THis cannot be so well perform'd by the Hygroscope made of an Oaten beard, if they, that have made use of them more than I, do complain with reason, that after some months (for I cannot tell you precisely how many) they begin to dry up and shrink; so that their sense of the varying degrees of the moisture of the Air is not so quick as before, and the informations they give of the degrees of it, especially towards the outmost bounds of their power to shew the Air's alterations, recede more and more from Uniformity. But the lastingness and other convenient qualifications of our Spunge making its capacity of doing service more durable, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one Year with the correspondent ones of another. And if the weight of the Spunge at a convenient time, when the temperature of the Air is neither considerably moist, nor considerably dry, be taken for a Standard, a person that should think it worth his pains, may, by computing how many dayes at such an hour, and how much at that hour, it was heavier or lighter than the standard, and also by comparing the result of such an account in one year with the result of the like account in another year, be assisted to make a more particular and near estimate of the differing temperature of

the Air, as to moisture and dryness, in one year than in another, and in any correspondent season or Month, assigned in each of the two years proposed. And how much the Collation or Continuance of such Observations, both in the same place and also in differing Countryes and Climates, may be of use to Physicians in reference to those Diseases, where the moisture and dryness of the Air has much interest; and the Husbandman to fore-see what seasons will prove friendly or unkind to such and such Soils and Vegetables; it must be the work of time to teach us, though in the mean while we have no reason to despair, that the Uses to be made of such Observations may prove considerable. And the rather, because if by help of the result of many Observations men be inabled to foresee (though at no great distance off) the temperature of a year, or even of a season, it may advantage not only Physicians and Plow-men, but other Professions of men, who receive much profit or prejudice by the dryness or excessive moisture of the seasons. And not to mention those who cultivate Hops, Saffron, and other Plants that are tender and bear a great price; such a foresight, as we are speaking of, may be of great use to Shepherds, who, in divers parts of *England*, are oftentimes much damnified, if not quite undone, by the rot of Sheep, which usually happens through excess of moisture in certain months of the year. And in order to the providing of foundations whereupon to build Predictions, it may not be amiss to register the number, bigness, and duration of the considerabler spots, that may at this or that time of the year happen to appear or be dissipated on or near the Sun, or to take notice

of

of any extraordinary absence of them, and to observe whether their apparition or dissipation produce any changes in the Hygroscope: Which Curiosity I should not venture to propose, but that (as I elsewhere note) I find, that eminent Astronomers have casually observed great dryenesses to attend the extraordinary absence or fewness of the Solar Spots. And those persons that are Astrologically given, may, if they please, extend their Curiosity in the use of this Instrument to observe, whether Eclipses of the Sun and Moon, and the great Conjunctions of the Superiour Planets, have any notable operation upon it.

USE III.

To discover & compare the changes of the Temperature of the Air made by Winds, strong or weak; frosty, snowy, and other Weather.

THis may conveniently enough be done as to winds, either by our whole Instruments or (perhaps better and more safely) by the Spunge alone, which may be taken off and hung by a string, for as long time as is thought fit, in the wind, and then restor'd to its former place. For I found by removing it into the wind, that it soon receiv'd a very considerable alteration in point of weight, as also it did when remov'd out of a room into a garden where the Sun shin'd; for though the season were not warm, it being then the Moneth of *January*; yet in three quarters of an hour the spunge lost the 24th part of its weight. We may also in some

cases usefully substitute to a Spunge a somewhat broad piece of good Sheeps-leather display'd to the wind. For this having, by reason of its thinness (or very small depth,) in proportion to its breadth, a very large *Superficies* immediatly expos'd to the wind, we found it to be notably alter'd thereby, in so much that half an ounce of well prepar'd Sheeps-leather, (that we had long imployed as an Hygroscope) being kept an hour in a place, where the Sun-beams might not beat upon it, did, in a strong wind, vary in that short time an eighteenth part of its original weight. But though I think it very possible to make such observations of the Temperature of particular winds, as will frequently enough prove so true as to be usefull, at least to those that live in the places where they are made; yet I am of opinion, that, to be able to settle Rules any thing general, to determine with any certainty the Qualities of winds according to the corners whence they blow, as from the East or West, North-east, South-west, &c. there will be a great deal of wariness requir'd; and he that has not some competent skill in Physicks and Cosmography, will easily be subject to mistakes in forming his Rules. To countenance which advertisement, I shall now make use but of these two Considerations, whereof the first is; That winds that blow from the same Quarter are not in some Countryes of the same Quality that they are in most others, the wind participating much of the nature of the Region over which it blowes in its passage to us. At the famous Port of *Archangel* they observe, that whereas a Northerly wind almost every where else without the Tropicks produces frost in Winter, there it is wont to be attended with a thaw, so as to make the Eeves to drop. Of which

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the reason seems to be, that this wind comes over the Sea which lyes North from that place; and on the contrary, a Southerly wind blowing over a thousand or twelve hundred miles of frozen land does rather increase the frost than bring a thaw. This was by the Inhabitants averr'd to the Russian Emperors Physician, who was more than once at *Archangel*, and from whom I had the Account. The Northern windes that are elsewhere wont to be drying, are said in *Egypt* to be moist. I remember Mr *Sands*, in his exellent Travells, giving an account of what he observ'd about the largest of the fam'd Egyptian Pyramids, has this considerable Passage; *Yet this hath been too great a morsel for time to devour, having stood, as may be probably conjectur'd, about three thousand and two hundred years, and now rather old than ruinous: yet the Northside most worn by reason of the humidity of the Northern Wind, which here is the moistest.* Sands *in Purchas's Pilgrimage.*

And 'tis yet more considerable to our purpose what I find related by Monsieur *de Serres* in his usefull book of Husbandry, since by that it appears, that even in not very distant Provinces of the same Kingdome the winds that blow from the same Quarter may have very differing Qualities and effects. For, speaking of the Changes of the Air in reference to Husbandry in several parts of *France*, he informes us, that 'tis observ'd, that in the Quarters about *Tholoze* the South-wind dryes the ground, and the North gives rains. Whereas on the contrary from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this last nam'd wind causes dryness, and the other brings

Lib. 6. Cap. 8. Sect. 3.

Theat. d' Agricult. Lib. 1. chap. 7.

moisture. And this may suffice for my first Consideration. My Second is this, That the vehemence or the faintness of the windes, though blowing over the same country, may much diversify its operation on the Hygroscope, and the same wind, which, when it blows but faintly, or even moderately, is wont to appear moist by the Hygroscope, may, when vehement or impetuous, make the Instrument grow lighter, discussing and driving away more vapors by the agitation of parts it makes in the Spunge, than is countervail'd by those aqueous Vapors that are brought along with it. But on such things as these I have not leisure to insist, and therefore I shall proceed to take notice in very few words of some other operations of differing weathers on our Instrument, and tell you, that Frosty weather often made the Hygroscope grow lighter even at night: Snowy weather which lasted not long, added something to the weight of the Spunge. And it has been observed that mists and foggy weather us'd to add weight to it, even notwithstanding Frost.

To which may be added an Observation made by my Amanuensis, who having a convenienter chamber than mine, (wherein a fire was daily made,) was diligent and curious to set down the changes of the Hygroscope that was left in his lodging; for this observation makes it probable, that a transient cloud in fair weather may be (for I say not, that it always is) manifestly observable by our Instrument. For by his Diary it appears, that the 9th. of *September* being for the most part a very fair Sunshiny day, though about ten a clock in the morning the Sun shone brightly, the Spunge began to preponderate, which unexpected *Phænomenon* made him

him look out at the window, where he diſcover'd a cloud that darken'd the Sun, but after a while that being paſt the balance return'd to an *Æquilibrium*. On this occaſion I ſhall intimate, that I have more than once or twice obſerv'd, eſpecially in Summer, that when the Air grew heavier, the Hygroſcope either continued at a ſtand, or perhaps, alſo grew lighter; as if, when ſuch caſes happen, the *Effluvia* that get into the Air, either from the Terreſtrial or ſome other mundane globe, were not fit like vapors to enter and lodge in the pores of the Spunge, and ſo were Corpuſcles of another nature, with which when we find by the Baroſcope that the Air is plentifully ſtockt, it may be worth while to obſerve, Whether any, and if any, what kind of Meteor, as wind, or Rain it ſelf, or Hail, or in the Winter Snow or froſt, will commonly be ſignified and produc'd.

USE IV.

To compare the Temperature of differing Houſes and differing Rooms in the ſame Houſe.

AS this is of great uſe both in reſpect of mens Health, eſpecially if they be of a tender or ſickly conſtitution, and in reſpect of conveniency for the keeping fleſh, ſweet-meats and ſeveral ſorts of wares and goods, and even houſhold-ſtuff, that are ſubject to be indammaged by moiſt air; ſo it is readily and manifeſtly derivable from our Inſtrument. For, by removing it into ſeveral Houſes or into ſeveral parts of the ſame houſe, and letting it

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ſtand in each a competent time to be affected with the temperature of the Air of that particular place, we have divers times obſerved a notable difference, as you may gueſs by the two or three Notes I met with among ſome old papers.

Octo. 13. [Three or four days agoe a piece of fine Spunge being taken out of a Cabbinet and clipt till it came to weigh juſt half a drachm in a nice pair of ſcales and a warm room, was afterwards remov'd into a neighbouring room deſtitute of a chimney, (and yet within 3 or 4 yards of a chimney ſeldom without fire :) This ſtatical Hygroſcope, conſiſting of the ſcales and the frame they hung on, was yeſterday night remov'd into the former room, and the Spunge was found to have gained 3 grains and an eighth or better, and conſequently more than a tenth part in reference to its firſt weight; but being ſuffer'd to ſtand in this warm room, in leſs than 12 hours it loſt a grain and about $\frac{1}{8}$ of its former weight, though the time it ſtood in this room were for the moſt part night and rainy weather.]

[We took a piece of very fine Spunge, which formerly had weigh'd juſt a drachm, but having been many months kept in a very warm room where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring houſe and weighing it in tender Scales, in the Evening 'twas found to want of a Drachm 4 grains and $\frac{3}{4}$ of a grain; and though there was a fire in the room and the Scales ſtood not far from it, yet, in a ſhort time, (the day being foggy and rainy,) the Spunge viſibly depreſs'd its Scale $\frac{3}{8}$, and the next morning was found to want but one grain and a half of a Drachm, ſo that it had gain'd about three grains and

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a quarter, and the following evening, being the second of *January*, it weigh'd one drachm a grain and almost half a grain. So that in about one natural day the Spunge had acquir'd six grains from the moisture of the Air, that is, a tenth part of its first weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains, but about 3 or 4 of the Clock in the afternoon it began to lose of that great weight, which diminished more by the next morning, the weather having chang'd that night and become somewhat frosty.]

In another paper I also find this Note. [The drachm of a spunge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room where fire is not wont to be kept, the weather being extraordinarily foggy: This morning being brought into the former room, though now the weather be clear (yet not frosty) it appears to have gain'd in weight about eleven grains; yet it soon lost 2 grains by standing in this room all the while in the balance.]

USE

USE V.

To observe in a Chamber the effects of the presence or absence of a fire in a Chimney or Stove.

THis is easily done, and the more easily if the room be small. For in such chambers I have often observed a moderate fire to alter the weight of the instrument, placed at a distance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not seasonably renewed with fresh fuel, the decay of it would, in no long time, begin to be discernable by the Instrument.

USE VI.

To keep a Chamber at the same degree, or at an assign'd degree, of Dryness.

SUpposing the alteration of weight in our spunge to depend only upon the degree of the moisture of the Air, the last named use will be but an obvious Corollary of the former. For, if a convenient part of the Room be chosen for the Hygroscope, and it be kept constantly there, 'tis easy, by casting ones eye on it from time to time, to perceive when 'twill be requisite to increase or moderate the fire, so as to keep the spunge at that weight it was of, when the temperature of the Air of the chamber as to dryness and moisture was such as was desired. I will not

not trouble you with some scruples, which I confess the consideration of this use of our Instrument suggested to me, because I have not now the leisure to discuss them. I had thoughts to try, whether and how far a good Quantity of salt of Tartar or even dryed Sea-salt, being kept in a closet or some closer room, might by imbibing lessen the moisture of the Air in it, but I did not perfect any observation of this kind. But I will add to what I have already referred to this sixth Head, that I have sometimes noted with pleasure, how manifest and great a change in the weight of our Spunge would be made, when the room was washed and a good while after, notwithstanding that a good fire was kept in it to hasten the drying of it.

Besides the hitherto mention'd uses of our Hygroscope, I know not whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of shewing us some difference betwixt steams of differing natures, as those of Water, spirit of Wine, Chymical Oils, and perhaps new kinds of substances (such as we have not yet taken notice of) in the Air, in which, I confess, I suspect there may sometimes be dispersed store of Corpuscles, that I do not yet well know what to think of. For I have more than once observed (not without some wonder) the Hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions or variations of it seem plainly to require: Whether unobserv'd Corpuscles perform'd this by making the other steams in point of figure, or size, incongruous to the minute pores of the spunge, and so unfit to enter them; or by dissipating or otherwise pro-

procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not sure, but by associating this instrument with the Thermoscope, Baroscope and some others that may be proposed, it might be so improved, as to help us to foresee divers considerable things, that either are themselves changes of the Air, or are wont to be consequences of them: As sickly and healthfull constitutions of the Air both as to Man and Cattle; and healthful, barren or plentifull seasons in particular places or Countrys; and perhaps also strong Hurricanes, Earthquakes, Inundations, and their ill effects, especially those accidents that depend much upon the surcharge of the Air, with other Exhalations and moist Vapors, which operate before sensibly upon our Instrument, and therefore may be discernable by it a good while before they arrive at that height that makes them formidable Meteors. And if it were but the foretelling approaching rain, this very thing may on divers occasions prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that swim up and down in the Air, than our senses do, so that I have been able to foresee a showr of rain, especially in dry weather, a not inconsiderable while before it fell.

See uses the and the

And here I should dismiss our subject, which I have already dwelt on longer than I design'd, but that remembring a caution I gave you when I was speaking of winds, I think it but fit to add two or three lines, to keep you from being by that Advertisement discouraged from endeavouring to make in the ge-

See the III use.

general such Hygroscopical observations, as may be reduc'd to *Hypotheses*. For, as I elsewere discours'd concerning *Barometrical* Theories, if I may so call them; so I shall here represent concerning *Hygroscopical* ones, that if a Theory or *Hypothesis* that is it self rational, be found agreeable to what happens the most usually in observation; it ought not lightly to be rejected or so much as laid aside, though sometimes we find particular Instances, that seem to call it in question. For 'tis very possible, that the Theory or *Hypothesis* may be as good as a wise man would require about so mutable a subject as the weather. And the Cause assign'd by the *Hypothesis* may really act suitably to what that requires, though a contrary effect insue by reason of that Causes being accidentally master'd and overrul'd by some more powerfull Cause or Agent that happens for that time to invade the Air. As we know that Tides do for the main correspond with the motions of the Moon, (whose *phases* are therefore argued from them,) and do generally ebb and flow at such times and in such measures as the Theory, that has been grounded on that correspondency, requires; but yet Seamen find, that in this or that particular harbor or mouth of a River, fierce contrary winds, great Land-floods and other casually intervening Causes, do sometimes both very much disturb the regular course of the Tides, and increase or lessen them.

FINIS.

A NEVV EXPERIMENT

And other INSTANCES OF THE

EFFICACY OF THE AIR'S MOISTURE.

Subjoyn'd by way of

APPENDIX

To His

STATICAL HYGROSCOPE.

By the Honourable *ROBERT BOYLE.*

LONDON,
Printed by *E. F.* for *R. Davis*, Bookseller
in *Oxford*, 1673.

ADVERTISEMENT.

THe Author had thoughts of illustrating the foregoing Paper with a Collection of Hygroscopical Observations, but though he several times begun Diaries of Occurrences of this Nature, as his Removes and other Avocations would permit; yet besides that these Impediments made him more than once break off his work, after he had continued it for a Month or two or longer, such unwelcome Accidents happen'd since the foregoing Tract was sent away, that he could not seasonably recover any competent number of Observations, and fears he shall never recover some of them, which he doubts not to have been, with many better upon various subjects, stoln away from him. Upon which occasion he thought fit to try, whether the following Paper might not be look'd upon as some amends for the missing of those Observations in whose room it is substituted.

A NEVV EXPERIMENT And other INSTANCES OF THE EFFICACY of the AIR'S Moisture.

SInce it may probably serve to recommend Hygroscopes to you, if that Quality of the Air, which these Instruments are usefull to give us an account of, be made appear to be more powerful, and have considerabler effects, than is commonly believed; it will not be from my purpose to present you here some Instances that have led me to think, that the Effects of the *Moisture* of the *Air* may be considerable not only upon mens Healths, but upon subjects far less tender, and less curiously contriv'd, than Humane bodies. But I hope, you will easily believe, that by the Moisture of the Air I mean not a meer and

abstracted quality, but moist Air it self, or rather those humid Corpuscles, (chiefly of an Aqueous nature,) that abound, and rove to and fro, in our common Air.

That the Moisture of the Air may have no small influence, and usually a bad one, upon mens healths, is that, which, though Experience did not so often teach us, I should venture to argue from what I have observ'd of the operation of moist Air upon the dry and firmly context parts of Animals, and even in those cases, where, for want of time or other Impediments, this Moisture cannot produce any sensible degree of putrefaction.

That the skins of Animals may be easily invaded by the moist particles of the Air, is the more probable, because of the numerousness of their Pores, which may be concluded from their hairiness, or their sweat, or both. And I formerly observed to you, that I found *Sheeps-Leather* to imbibe the moisture of the Air, and increase in weight upon it, as plentifully as almost any Body I expos'd to it.

But to shew you, that much closer Membranes, and which Nature made to be impervious to such a Liquor as Urine it self, may be affected by the Vapours of the Air, I shall add, that having purposely taken pieces of Bladders fine and well blown, and, as far as appear'd, of a very close contexture, and counterpois'd them in a good balance, I found, according to expectation, that they would considerably increase their weight in moist, and lose it again in dry, weather; so that I might have employed the most membranous part of a bladder (for I thought not fit to make use of the neck or the adjoyning part) to make a *Statical Hygroscope*.

And,

And, as for other membranes and fibres, I shall have by and by occasion to take notice, that even when they are strongly and artificially wreathed together into gut-strings, they may imbibe enough of the moisture of the Air to be broken by it. And, I remember, I formerly told you, that I had observ'd *Lute-strings* to grow heavier in moist Air.

And whereas *Bones* are by all confess'd to be the firmest and solidest parts of Animals, and as it were the pillars by which the fabrick is sustain'd; yet it seems, that even they may be pierc'd into, and sensibly affected, by the moisture of the Air. For I remember, that having caused the *Skeleton* of a humane body to be so made by a famous and very skilful Artist, that, by the help only of slender wires artificially order'd, the motions which the Muscles make of the bones of a living body might be well imitated in the *Skeleton*, I observed, that though in dry and fair weather the flexures of the Limbs might be readily made, yet in very moist weather the joynts were not easily bent, as if the parts were grown stiff and rigid; which seem'd to proceed hence, that moist particles of the Air, having plentifully insinuated themselves at the Pores into the Bones, had every way distended them, and thereby made the parts bear hard against one another, (which they did not at all before) at the Junctures or Articulations.

But it will be the more readily believed, that the Moisture of the Air may operate considerably upon the tender and curiously contriv'd Bodies of Men and other Animals, if, proceeding to the Observations I chiefly design, I make it appear, that the moistning Particles, that rove up and down in the

Air, are able to exercise a notable (and, if I may so call it, a Mechanical) force even upon Inanimate and Inorganical bodies: which may well suggest a suspicion, that *Hygroscopes* being the proper Instruments to discover a Quality in the Air, whose efficacy reaches farther than is commonly taken notice of, they may in time be found useful to divers other purposes, besides those that relate to the health of men.

That *Wood*, especially when it has been season'd, is a Solid of a strong and firm contexture, if it were not obvious by the daily use made of it in building Ships, Houses, *&c.* might be easily concluded from the weight or force requir'd to alter its contexture by making any considerable, or perhaps sensible, Compression of it. And yet, that *Wood* may suffer a kind of divulsion of a multitude of its parts, and be manifestly distended by aqueous Corpuscles getting into its Pores, I remember, I proved by this Experiment. I got a piece of sound and season'd Wood of about an inch (or an inch and half) in Diameter, to be by a skilful Artist made Cylindrical, and also a ring of some solid matter, as Brass or Ivory, to be exactly turn'd to fit this Cylinder, so that it might without much ease, or much difficulty, be put on and taken off again: Then we put the turn'd piece of Wood into fair Water, and left it to soak there for many hours; at the end of which it was visibly swell'd, and though I cannot now tell you, (for want of a Paper concerning that Experiment,) *how much* it was increas'd in Diameter, yet I well remember the increment was considerable, and that the ring, that was adjusted to it before, was manifestly too

little to be put again upon it, or with its Orifice to cover the whole *basis* of the distended Cylinder, which afterwards being dryed in the Air shrunk into a capacity of entring the ring again. And in this Experiment I took notice, that the great Intumescence of the Wood was not produc'd all at once, or soon after it was put into the Water, but it swell'd by degrees, and lay soaking there many hours before it arriv'd at its utmost distension, the aqueous Corpuscles requiring, it seems, so much time to insinuate themselves sufficiently into the Wood; which argues, that the internal parts were likewise affected, though, when even they came to swell, they had a good thickness of Wood about them to hinder their Dilatation.

I expect you should now tell me, that this distension of so firm a Body was made by Water it self, and not by the humid Vapours of the Air. On which occasion I might represent to you, that by the sweating (as men commonly call the adhesion of wateriſh drops to the surface) of polished marble and some other cold and smooth Bodies, that sometimes happens even in the Heat of Summer, if they be cold, and the ambient Air moist enough; it appears, that both in hot weather the Air may be plentifully stock'd with aqueous Vapours, and that these Vapours need to do no more than convene together to constitute visible and tangible Water. And on this occasion, if I were sure I had not told you of it already, I should subjoyn an Experiment which would detect the Vulgar error of those that think the adhering drops, lately mention'd, to come from some internal moisture deriv'd by its pression or percolation from the marble or the

Body they are fasten'd to; and at the same time I shall shew (what is not wont to be imagin'd) that in the Heat of Summer the Air is furnished with invisible and yet aqueous steams. The Experiment I long since try'd in Winter with Snow and Salt, included in a glass Vessel, and then put to dissolve in a balance. But because neither Ice nor Snow is at all easie to be come by among us in *England* in Summer, and because, at that season, the Air in fair weather is presum'd to be dry as well as hot, I chose, within some dayes of *Midsummer*, and in clear Sun-shiny weather, to make the following tryal.

We took a pint glass-bottle, and having put into it a convenient quantity of Water (for room must be left for the Salt) we plac'd them and four ounces of beaten *Sal Armoniack* in one scale of a good balance, and a counterpoise in the other, and then, putting the Salt into the Water, I observ'd, that though for a while the *Æquilibrium* remain'd, yet when the frigorifick mixture had sufficiently cool'd the outside of the Bottle, the roving Vapours of the Air, that chanc'd to pass along the surface of the Vessel, were, by the contact of that cold Body, arrested, and turn'd into a kind of a dew, which from time to time made the scale, that held the glass, preponderate more and more, and at length the drops growing greater and greater, ran down in small rivulets the sides of the Glass, and in less than an hour, (by my estimate,) the condens'd steams amounted to near a dram, which weight was afterwards much increas'd within about two hours more: Whereby it sufficiently appears, both that this dew came from without, (since if it had been a transuda-

tion,

tion, it would not have added weight to the scale that received it,) and that there is even in clear Summer weather a vast number of moist particles dispers'd through the Air, since, in about an hours time, such a multitude of them as the Liquor produc'd may be suppos'd to consist of, and may by Heat be actually resolved into, could in course come to touch so small a surface, as that of that part of so small a bottle which contain'd the frigorifick mixture. For the rest of the Vessels surface was not cold enough to condense the Vapours into Liquor. But to return to what we were saying of Wood swell'd by water; because, notwithstanding these Considerations, I am willing to allow, that the Experiment of the Cylinder does not fully come home to our purpose, and that I produc'd it not so much to *prove directly* the force of moist Air, as to *countenance* what I am about to say, by shewing what a sufficient number of aqueous Corpuscles may do in the solid wood they penetrate, I shall now add some instances of the force these particles may exercise upon Solids, when they invade them but in the form of Vapours.

That in this form the multitude, figures, and motions of these insinuating particles may inable them to display no small force in their operations on some Bodies, we have one Instance that often happens, though but seldome reflected on, in the breaking of the strings of Musical Instruments, first brought to a good Tension, upon the supervening of rainy weather. For the cause seems to be, that the Vapours that then wander through the Air, insinuating themselves into these strings, (which the Musician often forgets to let down or relax after having

ving skrew'd them up,) distend and swell them, and thereby endeavour to shorten them, and that so forcibly, that they not seldome break with a smart noise and great violence; which, because it happens without any visible efficient, men commonly think and say, that such strings break of themselves. But, to take no further notice of this popular surmize, if we consider how much weight some of those bigger strings, especially of Base Viols, that have been observed to break in rainy weather, will require to stretch any of them to a rupture, you will easily be induc'd to think that this operation of the moist Air exacts, and therefore argues, more than a languid force.

But here probably you will tell me, that the Instances you expected were concerning Wood, which is a far solider Body than gut-strings. To this I say, that the newly recited Instance belongs directly to the title of this Paper, and, being above referr'd to, ought not to be pretermitted. And, as to your expecting Instances concerning Wood, I might content my self to refer you to what is observ'd about the uneasie opening and shutting some doors well adjusted to the door-case in very rainy weather. But though this Observation favours my design, yet I had rather give you Instances in wood purposely and carefully season'd. And therefore I shall now inform you of these two things; *one* that I found by tryal (as I have elsewhere noted) that Wood counterpoised in a good balance would grow sensibly heavier in wet weather, and lighter again in dry; and the *other*, that, to satisfie my self yet further, I consulted an ancient Musician, to whom I had once been a Disciple, and a famous Organ-

maker,

maker, to know whether they had not obſerv'd that the wood it ſelf, *&c.* of Muſical Inſtruments would receive ſuch alterations from the moiſture of the Air, as might be diſcern'd by the Ear? Upon which inquiries, the Maſter of Muſick anſwer'd me, That though Metalline ſtrings will not change with the weather like Gut-ſtrings; yet Virginals (for inſtance) though furniſhed with wire-ſtrings, will for the moſt part of them, (for ſome he has obſerved to be ſo well ſeaſon'd that they are not alter'd by the weather,) be out of Tune in wet weather, the ſtrings generally then affording their notes ſharper than they ſhould or are wont to do. And the Organ-maker confeſs'd to me, that, upon great changes of weather, divers Organs would (after they had been long ago tuned) grow out of tune, and that not only the woodden pipes would be thereby ſwell'd, but the Metalline pipes untuned.

But if Bodies be of ſuch a Conſtitution as not only to admit but aſſiſt the operation of the moiſt Air, the penetrancy and efficacy of this may be found much more conſiderable than in the fore-going Inſtances. For there are ſome kinds of thoſe Marchaſites that yield Vitriol, which, whilſt they lye under ground, or are cover'd with the Sea-water, on whoſe ſhores they are in ſome places to be found, retain a ſtone-like hardneſs, and are often taken for meer ſtones; and yet ſome credible perſons that are converſant about Vitriol have caſually obſerv'd, that theſe, being expos'd to the Air, would in tract of time be ſo penetrated by the moiſt particles of it, though perhaps not meerly as moiſt, that (probably by the help of the Vitriolate Corpuſcles they met with among the ſtony matter) theſe hard and ſolid

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Marchasites are brought to swell so much as to burst. That this will happen to such kind of stones (though they be of a close and heavy nature) by the help of rain, Experience has perswaded me, and that it may also happen even to very hard and stone-like Marchasites, (for many are not such,) when they are meerly expos'd to the Air, I am apt to think upon some tryals of my own. For from shining Marchasites, though but kept in my Chamber window, I have had Vitriolate Efflorescencies that seem'd to be produc'd by the action of the piercing moisture of the Air upon the Mineral. And I remember, that very hard and heavy lumps that were of a Marchasitical substance, (though not at all glistering,) which seem'd to be stony, were so dispos'd to be wrought on by the Air, that though they were kept partly in my own chamber, and partly in other cover'd places, yet in no very long time they were so penetrated by the moist Corpuscles of the Air, that they were not only burst, but broken into many pieces; insomuch that many of them did of themselves fall off from one another, and several of the divided portions would easily be crumbled betwixt ones fingers. And of some of these I have observed with pleasure, that a Vitriolate substance was produc'd more copiously in their innermost parts than on or near their outside. So that, when I consider'd how great an external force would have been requisite to make such a Comminution of Minerals so solid and hard, 'twas obvious for me to look upon the Air's moisture, as capable, when it meets with fitly dispos'd Bodies, to exercise a far greater force than is wont to be conceived.

To these *Phænomena* I might add some others to the

the same purpose; but because the Marchasites, and other Bodies requir'd to the producing of them, are not easie to be come by, and the success often exacts a good length of time, I shall conclude this Paper by subjoyning a far shorter Experiment, that I devis'd not only to shew in general, that the moisture of the Air may have a considerable Efficacy, but to assist a *Virtuoso* to make some estimate in known measures of the Mechanical force of the Aerial moisture. And though I now find to my trouble, that I want some of the Notes that concern the Circumstances and the progress of the tryal, yet enough having escap'd to furnish me with the following account of it, what I shall set down may, I hope, at least put you in the way of repairing my misfortune.

Thinking it then probable, that Ropes themselves would considerably imbibe and dismiss the moisture of the Air, and that so as to shrink in rainy weather, though clogg'd with a weight fasten'd at the lower end, I was not discourag'd from attempting the following Tryal, by considering that the weight would stretch the Rope, and consequently hinder the presum'd effect of the Air's moisture to be perceived. For I suppos'd, that after a time this unusual stretch of the Rope would cease, and when the weight as such could not lengthen it any more, it would then be capable of being contracted or relax'd, according as the weather should be moist or dry, and so afford me a kind of Hygroscope. Upon these grounds I first caus'd a Rope that was about 20 or 22 yards in length, but of no great thickness, to have one of its ends fasten'd to an immoveable Body at a convenient height from the ground, and then caus'd a Pully to be so fasten'd to another stable Body at the distance

ſtance of 18 or 20 yards from the firſt, that the Rope, reſting upon the Pully, lay almoſt horizontally. But to the end of that part of the Rope, which from the Pully reach'd within two or three foot of the ground, was faſten'd by a Ring a Leaden weight of at leaſt fifty pound. To which was alſo faſten'd a light *Index* plac'd horizontally, whoſe end moved along an erected board, which by tranſverſe lines was divided into inches and parts of inches, reaching both a good way upwards and downwards, that the *Index* might within thoſe bounds have room to play up and down according to the alterations of the weather.

It being then Summer, this Tryal was made in a Garden, though partly under a Penthouſe, that the Rope might be more expos'd to the Air than it would have been within doors; and two or three dayes, if I miſremember not the time, were ſpent, before the weight had brought the rope to the utmoſt ſtretch it was able to give it, after which it began manifeſtly to ſhrink and lengthen according to the weather. And I find in one of my Notes, that once I look'd, when I was ready to go to bed, upon the ſuſpended weight, and mark'd how low it reach'd upon the divided board; and that a great part of the night having been rainy, looking again about half an hour after eight in the morning, I found the Cord ſo ſhrunk, that the weight was rais'd above five inches, and yet the day growing dry and windy, and ſometimes warm, the weight had at night ſtretched the Rope more than the moiſture had contracted it the day before.

Afterwards having procur'd a far greater weight, but therefore unapt to be near ſo much rais'd, I ſubſtituted

ſtituted it in the place of that formerly mention'd, and having ſuffer'd it to ſtretch the rope as far as it could, I made and regiſter'd ſome Obſervations, two whereof having been preſerved, I ſhall tranſcribe them juſt as I find them.

June the 4th. At half an hour after nine of the clock at night, I look'd upon the hundred pound weight that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board, where the bottom of the weight touched; I perceiv'd the sky a while after to grow cloudy and overcaſt, but without rain; wherefore going to view the weight again, I found it to be riſen a quarter of an inch or more, and, looking on my Watch, perceiv'd there had paſſed an hour and quarter ſince the mark was made.

June the 6th. Being not well yeſterday, the weight was obſerv'd by two of my ſervants, and it then reſted at the eleventh inch of the erected board. This morning about eight of clock I viſited it my ſelf, and found it to be riſen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and duſty. The weather being more overcaſt, within ſomewhat leſs than an hour afterwards I viſited the weight again, (ſome ſcatter'd drops of rain then beginning to fall,) and found it to be riſen about half an inch above the newly mention'd eighth mark. How much more the rope would have been contracted in ſuch laſting moiſt weather, as uſually happens in Winter, I cannot ſay, having been reduc'd to break off the Experiment, upon a removal, I was, long before that ſeaſon, oblig'd to make.

I am sorry I cannot add my other observations, but these I hope may suffice to let you see, that the force of the Air's moisture is not small, since it could raise such a weight as an hundred pound, especially considering the slenderness of the rope it affected. For having measur'd the Diameter near the weight, I found it (as one of my Notes informs me) to be but about the third part of an Inch.

'Twas $\frac{3}{10}$ *and* 4 *decimal parts of* $\frac{1}{10}$.

FINIS.

BOUND BY MALTBY, OXFORD